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1159

United States Circuit Court of Appeals

FOR THE NINTH CIRCUIT.

MARCONI WIRELESS TELEGRAPH COMPANY OF AMERICA,
Plaintiff-Appellant,

vs.

KILBOURNE & CLARK MANUFACTURING COMPANY,
Defendant-Appellee.

In Equity No. 71—on Marconi Patent No. 763,772.

TRANSCRIPT OF RECORD.

PLAINTIFF'S REPLYING AND SURREBUTTAL TESTIMONY, DEFENDANT'S SURREBUTTAL TESTIMONY.

APPEAL FROM THE DISTRICT COURT OF THE UNITED STATES—FOR THE WESTERN DISTRICT OF WASHINGTON—NORTHERN DIVISION.

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Tuesday, July 18th, 1916, 10 o'clock A. M., continuation of proceedings pursuant to adjournment. All parties present as at former hearing.

STATEMENTS OF COUNSEL.

Mr. Hughes: If the court please, when the court adjourned before, the defendant was proposing to take certain testimony respecting the Thompson transmitter and, I think, they suggested a desire to take some additional testimony in re- 8786
spect to the receiver, and the court indicated a desire to close up that aspect of the case. It was apparent, however, that to take that testimony it would be necessary to hold Mr. Betts and Mr. Farnsworth over until the following Tuesday, and hence it was determined that that testimony should wait until the adjournment of the court. I would suggest that we consider whether that testimony should not be offered at this time, or what is the pleasure of the court and counsel respecting it.

Mr. Skeel: If the court please, I naturally 8787
supposed after the adjournment that our surrebuttal testimony would all go in together, and I supposed that the surrebuttal on the Thompson transmitter and on the receiver would go in at the same time as the surrebuttal on the Simpson transmitter, if we would need any; and, furthermore, as stated to the court the other day, before the surrebuttal on the receiver goes in we desire to have the check-up tests that the time did not permit Mr. Thompson to make at the time of the other tests, and so I think it is best for the plain-

8788 tiff to proceed with its rebuttal on the Simpson transmitter at this time. The plaintiff was in the midst of its case when we adjourned.

I will say further, if the court please, while, of course, I had no idea that plaintiff would suggest that they would discontinue their case, and the defendant should, in broken lots, proceed with its case, that it would be difficult for us to do that even if the court should desire it, for a day or two, for this reason: that the postponement of this case from April 29th to July 18th broke up
8789 our arrangement for witnesses. Mr. Pickard was held in the East and unable to return and we felt under those circumstances that we would have to make some arrangement to have Mr. Kolster and Dr. Zennick, the German authority, here. Dr. Zennick has come, and Mr. Simpson and Mr. Kolster are on their way, but it is exceedingly difficult to get the Department to arrange for Mr. Kolster's leave of absence just at this time, so that that delayed Mr. Simpson and Mr. Kolster and they are now somewhere in the state of South Dakota, I presume, on the way here.

8790 Mr. Hughes: Well, it is not very important at what stage the testimony reserved by the defendant, which it certainly desires to offer, respecting the Thompson transmitter and the receiver in this case, should be given, I apprehend; I merely called attention to it because the court indicated a desire to sever in a way, or to close up the testimony respecting the Thompson transmitter and the receiver independently, and I assumed that that was all there would be, except what related to the Simpson transmitter, to be heard at this adjourned session. Of course if

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counsel are not ready I do not think it is material. The order of the introduction of testimony of proof is a matter in the discretion of the court and the convenience of counsel, where no prejudice is shown, would be recognized. I merely suggest, however, that I do not want at this time to acquiesce in counsel's designation of the testimony by the term "surrebuttal". That might be true as to the proposed testimony respecting the Thompson transmitter and, perhaps, as to the receiver. A different aspect arises respecting the
evidence concerning the Simpson transmitter, and 8792
that question is one that will have to be considered and determined at the proper time. But if counsel are not ready at this time, there are certain depositions which have been taken, and we would be glad to facilitate matters by going ahead with the reading of those depositions.

Mr. Skeel: I might say further, if we gave our testimony now on the receiver that we would have to put our witnesses on the stand twice.

Mr. Betts: All right.

The Court: I understand that the testimony
may proceed now on the part of the plaintiff. Now 8793
if these depositions are to be read, will you read them at another than open session, or could not they or would not they be used in the argument to the extent that is necessary to develop the thought that is contained in them, rather than taking the time of the court in open session to read the entire depositions?

Mr. Hughes: I went over the depositions yesterday with Mr. Betts, if your honor please, and it seems to me that in order that the court would properly comprehend the oral testimony

8794 which will be later given on both sides, it will doubtless be necessary to read them.

Mr. Betts: I think also by so doing it may shorten the deposition of Mr. Waterman or any other depositions.

Mr. Skeel: Of course I will desire the same rights with respect to our depositions.

Mr. Betts: Certainly.

Mr. Skeel: I made this suggestion for the reason that it was the plaintiff's own suggestion, that we save the depositions for argument at the
8795 time we offered the original depositions of Mr. Stone and Mr. Pickard, but I have no objection.

The Court: It is my universal practice to read these depositions anyway.

Mr. Hughes: I understand that, and also that your honor would read the transcript of the testimony, but in going over these I do not see how the other testimony will be thoroughly comprehensive to the court unless these depositions are read.

OBJECTIONS TO PLAINTIFF'S REPLYING DEPOSITIONS.

8796 Mr. Skeel: Before Mr. Betts starts to read the depositions, I wish to make a general objection to the admission in evidence of the first deposition, which is the deposition of Professor Chaffee, an instructor in Harvard University. I do not know that the court will want to pass on this until he has read the entire deposition or not, but I wish to call attention to the point at this time, in order that my objection may be timely.

It appeared that certain observations with the aid of the Braun tube were made by Professor

 8797

Chaffee, and that in work he was assisted by a student, and also by a Mr. Washington. It appeared in the course of the testimony that Mr. Washington is now in business for himself, connected with the firm or the company of Cutting & Washington Company, which is a company engaged in manufacturing radio telegraph apparatus and is a competitor, so stated, of the Kilbourne Clark Company.

On page 54 of the deposition the witness stated that he had no relation with the Cutting & Washington Company other than having sold a patent 8798
to the company, which was a patent for a radio telegraph transmitter. The witness further testified that the transmitter was capable of being operated with half an oscillation in the spark circuit.

This objection to the deposition of Professor Chaffee is made for the reason that in cross examination he refused to answer interrogatories and questions which are material to this issue, and on this ground the entire deposition should be excluded.

To illustrate to the court the materiality of the questions which Professor Chaffee refused to answer on instruction from counsel, I will read just 8799
a few of the questions which he so refused to answer and which in my judgment bear very strongly upon the question of his competency in this case.

On page 56 this question was asked him:

"Q. As to your transmitter you referred to a moment ago, you have operated that also with an alternating current supply?"

Then Mr. Peters said—I might say that Mr.

Peters is an associate of Mr. Betts, and was taking the deposition—

"Mr. Peters: The witness is instructed that he need not discuss the operation of his own apparatus unless he so elects or unless he is instructed by the court to do so."

And then the witness replied:

"I do not care to answer."

On page 57 he was asked this question:

8801 "Q. Has the Cutting & Washington Company any relation with the Marconi Company? A. Nothing so far as I know."

On page 59 he admitted that this company was a competitor of the Kilbourne Clark Company, and then on page 64:

"Q. You, I take it, consider that the transmitter of your own design is better than Mr. Simpson's transmitter in respect to the limited or short duration of action of the primary? A. I do.

"Q. Has your transmitter had any extensive commercial use?

8802 "Mr. Peters: You need not answer unless instructed by the court.

"Q. I take it you have completely disposed of your interest in the transmitter?

"Mr. Peters: The same objection and instruction.

"Q. From the fact of what you said about having sold the patent to the Cutting & Washington Company. (That relates to a former question where he was asked if he had completely disposed of his interest in the transmitter.) A. I decline to answer."

And then on page 85:

"Q. Did your colleague, Professor Pierce, of Harvard University, have a part ownership interest in your Chaffee gap radio telegraph transmitter? A. Yes.

"Q. And you have understood that he sold out his interest to the Cutting & Washington Company?

"Mr. Peters: Same objection.

"A. I decline to answer.

"Q. Have either you or Professor Pierce any stock interest in the Cutting & Washington Company? A. I decline to answer." 8804

And then on page 91:

"Q. Did either you or Professor Pierce receive cash from the Cutting & Washington Company for your interest in your transmitter patent? A. I decline to answer."

Now, those questions and answers are merely given by way of illustration of a number of the same character, which questions were asked for the purpose of showing the interest of the witness in the suit in controversy. The witness declined to state whether or not he had any stock interest 8805
in the Cutting & Washington Company, to which he had sold his patent for a transmitter, which was a transmitter which was to be in competition with that of the defendant in this case.

I just wish to cite four authorities on that point, without reading from the cases, all of which are to the effect, and I think it is the universal rule, that if a witness refuses to answer material questions his entire deposition may be stricken:

83 Federal, page 614. The title of the case is

"Thompson-Houston Electric Company vs The Jefferey Manufacturing Company."

Also the case of "New York T. & M. Company vs. Green," 38 Southwestern, page 31.

Also the case of "H. & T. C. Railroad Co. vs. Shirley," 54 Texas, page 143.

I will just read a sentence from that case, as follows:

> "The authorities are that the omission to answer or the refusal to answer is fatal to the entire deposition."

8807

Also the case of "Smith vs. Griffith, 38 American Decisions, page 639.

In further support of my motion I just wish to call the attention of the court to this fact, that in this business of radio telegraphy, of course the commercial possibilities are very great. The question of whether a witness is disinterested or whether he is biased or whether he is influenced by any consideration of gain to himself, is something that is extremely material in this case. The testimony of the witness absolutely prevented the defendant from ascertaining to its satisfaction in-
8808 formation which would have guided the court in that respect.

The Court: I will dispose of this afterwards; the deposition may be read.

Mr. Betts: I just want to make one observation in reply, that when Mr. Farnsworth reads the whole of the cross-examination of Dr. Chaffee to you, you will see that Dr. Chaffee testified that the Marconi Company, the plaintiff here, had no interest whatsoever in the Cutting & Washington Company; and while I have not got the citation at hand, my recollection is that the case of the Thompson-Houston Electric Company against H. W. Jahns Manufacturing Company, a case in the Southern District of New York, decided by His Honor, Judge Lacoe, upheld the right of a witness to decline to answer certain cross questions, and refused to strike out the deposition. I can submit that authority to your honor as soon as I have an opportunity to look up the citation.

Stipulation as to Exhibits of Marconi Deposition. 8810

Mr. Betts: I will first read, if the court please, a stipulation signed between the counsel for the respective parties in the suit at bar, as follows:

"It is hereby stipulated that the deposition of Guglielmo Marconi, taken herein at London, England, on May 29, 1916, and now in the Clerk's office of this court, may be opened, published and filed.

It is further stipulated that the drawings, documents, newspaper articles, charts, tapes and other papers and exhibits referred to by said Marconi 8811
in said deposition as being in the possession of his counsel, Mr. Betts, in New York, and as having been produced in the case of this plaintiff against the National Electric Signaling Company, copies of which have been furnished defendant's counsel, may be considered as part of said Marconi's deposition and may be offered in evidence by Mr. Betts at the trial of this case with the same force and effect as if they had been produced by said Marconi in giving his said deposition and as if they had been attached to said deposition and formed

8812 Stipulation as to Exhibits of Marconi Deposition.

a part thereof and forwarded to this court by the officer before whom said deposition has been taken.

Said exhibits are the following:

Diagrammatic sketches or drawings, figures 1 and 2, of apparatus of re-issue patent 11913, as first tested;

Article from London Times of September 23, 1896, report of proceedings of British Association for the Advancement of Science;

Article from the London Daily Chronicle for December 14, 1896;

8813 Article from the London Electrical Engineer for December 18, 1896;

Preece article from the London "Engineer" of June 18, 1897;

Two British Channel Tapes of May, 1897;

Report of tests for the Italian Government published in the Italian Naval Reports,—"Revista Maritiana," for August, 1897;

Report of official tests by Lieut. Blisch of the United States Navy, published in the proceedings of the Naval Institute;

Article published in 'The Daily News' of St. John's, Newfoundland, of December 16, 1901;

8814 Article from the 'Evening Herald' of St. Johns's, Newfoundland, of December 16, 1901;

Extracts from minutes of the meeting of St. John's Municipal Council on Friday, December 20, 1901;

Tapes of tests on SS Philadelphia and chart showing positions at which the signals were received."

I will now, if the court please, read the deposition taken by the plaintiff under the order of this court dated June 1st, 1916, begun at the Cruft

High Tension Laboratory in the Harvard University, at Cambridge, Massachusetts, on the 5th day of July, 1916, pursuant to notice.

(Here counsel for plaintiff reads the direct examination of Dr. E. L. Chaffee. See Transcript, vol. 5, pp. 2964-3006.)

Reading of Plaintiff's Replying Depositions.

Mr. Betts: I presume now that Mr. Farnsworth would desire to read the cross-examination.

Mr. Skeel: If the court please, the cross-exam- 8816
ination takes from page 46 to page 110—about 65 pages. I may say that in argument we will desire to use not to exceed ten pages, and the defendant is perfectly willing to waive reading the cross-examination, unless plaintiff desires the whole of it should go in—I should be very glad if the plaintiff desires it—but I consider that there is a good deal of chaff with the wheat, and it takes a good deal of time to cover that.

Mr. Betts: I would agree with you were it not for the fact that in your objection made to the court this morning you segregated certain ques-
tions in cross-examination from their context, 8817
which might leave the impression in the mind of the court that this witness was not an impartial witness, while if those questions which you have called to the attention of the court had been right in connection with their context in the whole cross-examination, it is clear to my mind that your objection is not well founded.

The Court: I will state to counsel on both sides that I can read the cross-examination for myself, and I can read it in less than half the time that another person might take—I can read it over in much less time than you can stand and read it.

Mr. Skeel: Your honor, I have no objection to Mr. Betts reading it if he desires to. I might say that the principal part that we will use in argument, that is the part that goes with the diagrams which Mr. Chaffee drew on cross-examination.

Mr. Betts: You do not need to read it now if you want to refer to it in the argument.

Mr. Skeel: And you may read any part of the cross-examination which you may deem essential,
8819 Mr. Betts, either now or this afternoon.

Mr. Betts: There are certain passages of the cross-examination which I think should be called to the attention of the court, particularly in view of the defendant's objection, but as your honor has indicated that you have proposed to and will no doubt read it.

The Court: I expect to read all of this cross-examination. I will do that—I usually do that after court hours.

Mr. Betts: Then I will briefly say that the passages which I wish to call your honor's attention
8820 to are these: there is an endeavor evidently to make it appear in the cross-examination that Mr. Weagant and Mr. Waterman and Mr. Shoemaker did something, or might have done something to this transmitting set during or prior to the tests conducted by Dr. Chaffee; but Mr. Chaffee points out in his cross-examination that he alone conducted the tests; that nothing was done by Mr. Waterman or Mr. Weagant except under his directions; that he alone stated and advised the tests which should be made by him, without any suggestion of Mr. Waterman or Mr. Weagant.

 8821

Mr. Hughes: It would be unfair to the plaintiff to have a ruling by the court on the deposition if that ruling should be adverse to us, after the testimony in the case was closed; in other words, that question should be determined at this stage of the proceedings. The court will recognize the importance of that, because if the deposition were suppressed we should be advised at this time. Personally I have no apprehension upon the matter, but if counsel has presented the objection I think it should be determined by the court before we are
required to go beyond these depositions. We have 8822
no objection to having the balance of the depositions read at this time, but before we proceed further in the matter or get to the point of resting our case, a ruling should be had upon that objection, as it goes to the whole deposition.

The Court: The court could not rule upon it until I have read the cross-examination.

Mr. Hughes: Perhaps, to save time, the court may be able to read it during the noon hour and sometime today have the matter heard and determined on the objection. Will that be satis-
factory to the court? I think, in other words, we 8823
should know what the ruling of the court is on this deposition, since it is a motion on the whole deposition.

The Court: Yes.

Mr. Hughes: Before we proceed further. I think if Mr. Betts goes on with the other depositions until noon, the court can read the cross-examination, probably, during the noon hour, that is probably during the recess.

Mr. Skeel: I am perfectly willing to start in and read the depositions now; I simply made that suggestion.

Mr. Hughes: And I simply suggest that the question should be passed on before we go further with our testimony.

Mr. Betts: I may say this in regard to these objections to these questions; the questions were obviously improper questions, since your honor will see in reading the cross-examination that they were inquiring into the private matters of this witness.

8825 The Court: I will take that up with that point in mind.

Mr. Betts: The re-direct examination is very short and I would like to read it to your honor.

(Counsel for plaintiff proceeds with the reading of the redirect examination of Dr. E. L. Chaffee. See Transcript, vol. 5, pp. 3061-3066.)

I do not believe it is necessary for me to read the rest of this re-direct examination, except to say that it is shown that the spark gaps were sealed by Dr. Chaffee with his seal——

Mr. Skeel: I object at this time to any discus-
8826 sion of the evidence. I have carefully refrained from doing that myself.

The Court: I will read the re-direct examination myself.

Mr. Betts: Now, I will begin reading the next deposition, which is on page 112, being the deposition of J. H. Morecroft. (See Transcript, vol. 5, p. 3067.)

(Whereupon counsel for plaintiff reads the direct examination of J. H. Morecroft.)

That closes the direct examination—do you wish to read the cross-examination?

Mr. Skeel: No.

Mr. Betts: Then I will read the re-direct examination, as there are only a few questions. (See Transcript, vol. 5, p. 3085.)

(Recess taken to 2 o'clock P. M.)

AFTERNOON SESSION, 2 P. M.

The Court: I have read the cross examination of Dr. Chaffee during the luncheon hour, and I think the motion to suppress the deposition of Dr. Chaffee will be denied. The one or two objections made that perhaps might have had some bearing upon the weight of the evidence produced were to questions that were answered, and those that were not answered, I think the objections to those questions were well taken, and exception may be noted, of course, to the court's ruling. 8828

Mr. Betts: If the court please, I will now continue the reading of the direct examination of Dr. Coffin, of the College of the City of New York. There are a lot of objections, I will not take the time of the court to read; page 142, about the bottom of the page. 8829

(Whereupon Mr. Betts continues reading the direct examination of Dr. Coffin. See Transcript, vol. 5, p. 3087.)

Mr. Betts: Do you care to read the cross examination?

Mr. Farnsworth: I do not.

Mr. Betts: I now pass, if the court please, to the deposition of Charles R. Cross, beginning at page 154.

8830 Offer of Exhibits.

(Whereupon Mr. Betts reads deposition of Charles R. Cross. See Transcript, vol. 5, p. 3113.)

Mr. Betts: Will you read the cross-examination?

Mr. Skeel: Yes, I will read the cross examination. It is only three pages.

(Whereupon Mr. Skeel reads cross-examination of Charles R. Cross. See Transcript, vol. 5, p. 3126.)

8831 Offer of Exhibits.

Mr. Betts: At this point—I do not know whether it is necessary—but I should like to offer in evidence under the stipulation which is spread upon the record, the exhibit attached to the deposition of Mr. Marconi, which has been published, and ask that this exhibit be marked plaintiff's exhibit No. 66, A to L.

(Exhibits above referred to received in evidence and marked Plaintiff's Exhibits 66-A to 66-L.)

I also would like to offer in evidence the ex-
8832 hibits referred to in the depositions of Messrs. Chaffee, Morecroft, Coffin and Cross, read in evidence, and ask that they be marked plaintiff's exhibit 67-A to 67-E.

(Exhibits above referred to received in evidence and marked Plaintiff's Exhibits 67-A to 67-E.)

I also offer in evidence the deposition of Guglielmo Marconi, taken under commission under order of this court, and which has been returned and by stipulation opened and published, to be read hereafter. (See Transcript, vol. 5, pp. 2937-2939.)

 8833

The Court: Yes.

Mr. Skeel: So far as the deposition of Mr. Marconi is concerned, no objection is made to the deposition or to any of the exhibits. So far as the depositions of Professor Chaffee and the others are concerned, we renew the objections spread upon the record by Mr. Farnsworth at that time.

The Court: Yes.

Mr. Skeel: Particularly in view of the fact, as shown by the testimony of the other witnesses present at the taking of the tests, that the transmitter was not in normal operating condition at 8834
the time of the tests.

The Court: Yes.

Mr. Skeel: Particularly in view of the fact, as shown by the testimony of the other witnesses present at the taking of the tests, that the transmitter was not in normal operating condition at the time of the tests.

The Court: That would be a matter of weight rather than of admissibility.

Mr. Skeel: Without waiving the objections to the deposition of Professor Chaffee, we offer in evidence the sketches made by him upon cross examination, and also the sketches made upon the 8835
cross examination of Morecroft, Coffin and Cross. We also offer in evidence two boxes of photographs taken July 3rd and 4th by Dr. Chaffee, Box 1 containing fifteen negatives and Box 2 containing sixteen negatives. These have all been marked by the notary.

Mr. Betts: No objection to any of the exhibits offered.

The Court: All right, admitted.

(Sketches and boxes of photographs above referred to received in evidence and marked Defendant's Exhibits.)

Mr. Betts: I understood this morning that Mr. Skeel or Mr. Farnsworth wished to read the depositions of the witnesses they called under the order of this court of June 1st, explanatory of the depositions of Dr. Chaffee, Cross, Coffin and Morecroft.

Motions and Ruling on Order of Proofs.

8837 Mr. Skeel: I will do that, if the court please, in our regular surrebuttal. I think the plaintiff should finish the testimony, the rebuttal they have in mind.

Mr. Hughes: I do not quite understand the use of the terms "rebuttal" and "surrebuttal."

The Court: I do not think we care about the terms. We will proceed and receive the testimony.

Mr. Hughes: Well, of course we do not care anything about terms, but we do care about the proof.

The Court: When we get to the proof then we
8838 will determine whether it should be received or not.

Mr. Hughes: Yes, but, if the court please, it is a matter of some consequence to know whether we are going to be allowed to occupy the position of the plaintiff, or whether in respect to the Simpson mercury valve transmitter we occupy the position of the defendant; whether as plaintiff we have a right to close the testimony or not, and I think those are questions that should be determined now.

The Court: Very well, I will determine them now, if you desire.

Mr. Hughes: The thing that I want to have determined at this time is the position that the parties occupy with respect to the issue arising over the Simpson transmitter, so-called Simpson mercury valve transmitter.

The Court: I think, so far as the issue in this
case is concerned, that we have just a little di-
gressed from the order in which the proof should
be received, by reason of the declination of the
plaintiff to offer testimony in the first instance 8840
with relation to this Simpson mercury valve
transmitter, and it was upon the motion of the
plaintiff that the court granted further time to re-
ceive testimony with relation to that, and I think
that we ought to proceed and receive the testi-
mony. Now, on the part of the plaintiff with re-
lation to this Simpson mercury valve transmitter,
if there is anything in that testimony that should
be met by the testimony of the defendant, which
has not been covered heretofore, why then the
court would receive it. If there is anything in the
testimony which is offered by the defendant that
should receive further elucidation, the court per- 8841
haps should receive that, and so far as the burden
of proof is concerned I do not think that that is
a matter to be determined now upon the order in
which this proof is to be received. Since we have
gotten into this situation we will simply receive
all the testimony that is offered upon the issues
and then determine the burden of proof on the
consideration of all of the evidence which is pre-
sented upon the facts as they may be established.

Mr. Hughes: The purpose of the inquiry, be-

8842 yond what the court has intimated, is this; we should be so advised that we may know whether we at this time should offer the evidence that we have to offer in support of our contention of infringement, and, at a later time, when they have introduced all their defensive testimony—and certainly they would be entitled to defend as against our proof of infringement—then have an opportunity to call our witnesses to testify in respect to matters which are strictly rebuttal of all of the testimony that they shall have offered,
8843 whether heretofore given or hereafter given.

The Court: I think, as I said a moment ago, that the plaintiff should proceed now and present its testimony, and if there is anything that should be met by the defendant by way of showing that these demonstrations were not properly made, or anything that should be offered in relation to that, why that testimony should then be received on behalf of the defendant.

Mr. Hughes: I call the court's attention again to the position of counsel for the defense that the evidence it proposes to offer in rebuttal, and to the position that we shall take here and
8844 upon appeal that by that attitude and a ruling supporting it they assume the burden. I want them to understand that that is and will be our contention, if that position is taken.

Mr. Skeel: I am perfectly willing you can contend that or anything else you want to, but I do not agree to it.

The Court: As far as the burden is concerned and the use of those terms "Rebuttal" and "Surrebuttal", there will be no emphasis placed on them now. What we are concerned about is to

 8845

receive testimony and then determine what the testimony establishes, and where the law places the burden under the issues as they are framed. You may proceed.

Mr. Betts: If the court please, notice was given us on the 7th of July that the defendants would call as witnesses explanatory of the examination of Messrs. Morecroft, Coffin and others, Mr. Pickard, Mr. Simon, Mr. Stone, Mr. Zennick, Mr. Eastham, and one or two others. That testimony was to be taken in New York under the order of this
court. The defendant took that testimony insofar 8846
as it related to the calling of Mr. Pickard, Mr. Stone, and Mr. Simon. Now, if they have any further testimony explanatory to these Cruft high tension laboratory tests other than the witnesses whom they then notified us they were going to call, and I submit that they ought to call them now, so that when we put in our testimony, under Your Honor's ruling of putting it all in now in what you may call rebuttal or anything else, it is immatrial to me, that we can answer all of it at one time.

The Court: If there is any other evidence here
that should go in— 8847

Mr. Betts: They have notified us they were going to call Mr. Zennick.

The Court: (Continuing.)—We should receive it now, any testimony that was taken at New York, or contemplated in the postponement, should be received now.

Mr. Skeel: The testimony, if the court please, that was taken in New York—we were only allowed two days of the entire time of the continuance of this trial—was simply the testimony of the witnesses who saw the experiment in the Cruft Labor-

8848

atory. Now, in two days' time we did not have time to take the testimony of more witnesses, and that testimony will be offered as part of our surrebuttal. It includes the small amount of testimony given by Mr. Pickard in surrebuttal on the receiver and on the Thompson transmitter also.

The Court: That deposition is here?

Mr. Skeel: That deposition is here, and it is extremely short.

The Court: Well, we had better receive all the depositions that are here that were taken there.

8849 Mr. Skeel. I may say that that testimony, the witnesses in New York, was itself surrebuttal. It was surrebuttal testimony in itself.

Mr. Betts: I do not agree with you.

Mr. Skeel: I have no objection whatever to reading that testimony.

The Court: I think we ought to receive it. As I said a moment ago, those terms have no—in a hearing of this kind it does not carry any emphasis as to who has the first or last.

Mr. Skeel: The fact that I offer in evidence the testimony of Mr. Pickard there, which was taken
8850 unexpectedly upon very short notice, does not preclude our taking testimony in surrebuttal proper, any surrebuttal testimony upon the receiver, because I want the court to understand that it was definitely understood that we were to have short tests on the receiver to check up the tests that the plaintiff made.

Mr. Betts: I beg your pardon, it was not so understood by me.

Mr. Skeel: Well, I will make a motion now that either now or at the proper time we be permitted to have receiver tests.

 8851

Mr. Betts: We will take that up when you make your motion.

Mr. Skeel: I will make my motion now.

Mr. Hughes: Why not settle one thing at a time?

Mr. Skeel: The plaintiff is simply trying to make us put in our case piecemeal.

Mr. Hughes: No, if the court please, I thank that—

Mr. Skeel: (Interrupting.) I will put in those depositions and then I will make my motion for the receiver tests when the plaintiff finishes its case, 8852
and we will dispose of this in short order.

The Court: (Cannot we proceed with some other phase of this case?

Mr. Betts: Your Honor, as I understand it, has ruled that we should proceed to put in such evidence in reply—or any other term you please to use—now we want to know whether we are to answer Mr. Zennick, Mr. Simpson and Mr. Kolster and Mr. Eastham in regard to these tests at the Cruft high tension laboratory on what they have notified us and in regard to which they have taken certain evidence. We do not want to make a piece- 8853
meal job of this.

The Court: Now, let me understand this.

Mr. Skeel: This is the situation; when the trial concluded here last April the plaintiff was in the midst of its rebuttal.

Mr. Skeel: I say, when the trial was finished here on April 29th the plaintiff was in the midst of its rebuttal. It finished its rebuttal on the receiver and on the Thompson transmitter, but it offered no testimony upon the Simpson transmitter. It asked for a continuance of sixty days and

8854 asked if depositions could be taken in the East, to which I consented, and stipulated that the plaintiff might take depositions in the East. Stipulations were then entered into and the plaintiff was allowed up to and including the 8th of July to take depositions, and the defendant was allowed the 10th and 11th of July to take its surrebuttal depositions. Now, the plaintiff is still in its rebuttal. and we have not yet commenced our surrebuttal. What the plaintiff is endeavoring to have us do is to put our surrebuttal in so that they can answer surrebuttal in rebuttal.

8855 If the court please, we may still have to have some more surrebuttal if we adopt that course of procedure. We expect to have one more short period of testimony and to finish, and if we are compelled to put in our surrebuttal now, and then the plaintiff goes on and makes a lot of new assertions and changes its positions on some of the issues in this case, we may have to testify again, which we do not want to do. My suggestion is that the plaintiff proceed and finish its rebuttal, and we will then put in our surrebuttal, and if there is any new matter which we bring up, which I am

8856 confident there will not be, certainly plaintiff would be entitled to its final reply.

The Court: When this case was closed on the part of the plaintiff the plaintiff had not introduced any testimony with relation to the Simpson mercury valve transmitter. The defendant stated in open court that it would offer evidence, and the court held that that was within the issue. The defendant then upon the presentation of its case did present testimony upon the Simpson mercury valve transmitter, and the case was practically closed with relation to the Thompson transmitter and receiver. Then on motion of the plaintiff the stipulation was entered into which has just been referred to by you, Mr. Skeel, and the court postponed this trial so far as the Simpson mercury valve transmitter was concerned for the purpose of giving the plaintiff opportunity of taking testimony with relation to that. The Thompson matter was practically closed except some further testimony, I believe, you desired to present.

Mr. Skeel: Yes, on the Thompson transmitter very briefly, and the receiver, a few tests, but very brief tests. 8858

The Court: Now, both sides have taken testimony. The plaintiff has taken all of its testimony and the defendant has taken a part——

Mr. Hughes: (Interrupting.) No, the understanding was——

The Court: I mean you have taken all of your testimony in New York, and have some other testimony to present here.

Mr. Hughes: Yes.

The Court: Now then, the plaintiff has read the depositions taken in New York, and we are now at the point where we want to know whether these witnesses you are going to call will testify with 8859
relation to the matters directly placed in issue by reason of the Simpson mercury valve transmitter, or whether they shall likewise be interrogated with relation to the testimony that has already been offered by the defendant, is that the item?

Mr. Betts: In regard to the Simpson mercury valve transmitter.

The Court: With regard to that?

Mr. Betts: Yes.

The Court: I can readily see if we go into both of those issues that there possibly might be some confusion, if the defendant expects to offer some further testimony as original testimony. As I said awhile ago. those terms of "rebuttal" and "surrebuttal" do not apply here in the situation we are in now, because no evidence has been offered on the part of the plaintiff with relation to the one——

Mr. Skeel: Except its deposition.

The Court: (Continuing.) Except today upon this further hearing. Now, you expect to offer some further original testimony.

Mr. Skeel: No, none whatever. The testimony
8861 with relation to the Simpson mercury valve in New York simply anticipated as far as possible what the patent experts would have to say in answer to Mr. Waterman.

The Court: Then the only further testimony you would expect to offer with relation to the Simpson mercury valve transmitter would be testimony which would be explanatory of these various tests that were made.

Mr. Skeel: The conclusions drawn from them, yes. So far as the tests are concerned we are not greatly concerned about the tests. We draw directly
8862 opposite conclusions from those drawn by Mr. Waterman.

The Court: What is this motion that you suggested awhile ago you were going to make, what is that?

Mr. Skeel: In regard to the receiver?

The Court: Yes.

Mr. Skeel: Why, simply this, if the court please, the plaintiff in its rebuttal held certain receiver tests.

The Court: You mean in its testimony?

Mr. Skeel: Yes, in the rebuttal testimony of the plaintiff, they based that testimony upon certain tests of the receiver.

The Court: When you say rebuttal testimony, what testimony do you refer to?

Mr. Skeel: I refer to the testimony taken in open court last April.

The Court: Yes.

Mr. Hughes: The term "rebuttal" I agree to there, in respect to the transmitter and receiver.

Mr. Skeel: Now, that was in relation to the Thompson transmitter and the receiver, that testimony was based upon a number of tests conducted 8864
by the plaintiff in the presence of the Assessors. We desire to conduct certain brief check-up tests on the receiver. We do not deem it necessary in relation to the transmitter, but we do deem it necessary to conduct certain brief check-up tests in relation to the receiver, and we are willing to do that any time and close the case on that point.

The Court: Now, as I said awhile ago, I think, in view of the status of this case and the condition of this record, that the only orderly way to proceed now is for the plaintiff to present its testimony bearing upon infringement, and likewise to meet 8865
the testimony that has already been presented by the defendant. The plaintiff has had the advantage of that testimony, and I think that now the testimony should be received. Now, if there is any testimony that the plaintiff or the defendant desires to offer that will answer that testimony or explain it, why then it will be proper to receive it. I understand from counsel that there will be no further original testimony.

I think that the only orderly way to proceed now is as I have indicated. We might well when a

8866 witness is on the stand receive all they have to offer upon the case and then if there is anything that needs to be explained that could properly be received, the court can receive it, and the same opportunity will be open to the plaintiff. If there is anything new that should be met the court ought to receive it.

The Court: If there is anything that is new that needs to be answered for the purpose of properly placing the issue before the court that could not have been disclosed by the witness when he was
8867 upon the stand.

Mr. Hughes: And then the court would hold that if Kolster, for example, or Mr. Simpson, go upon the stand hereafter, Mr. Waterman could not be called upon as to any matters that involve the testimony they formerly gave. That puts us in rather an awkward position.

The Court: You gentlemen want to limit and circumscribe entirely too much. What we want here are the facts which bear upon this issue, and I think we ought to proceed now.

Mr. Hughes: I think the court misconstrues our
8868 position, and I want to take an exception to the ruling of the court, and also to say that as we construe the matter the defendant is asking here, and by the ruling of the court is accorded the rights of a plaintiff without the burden of a plaintiff.

Mr. Skeel: We are not asking anything of that kind. If there is any new matter that we put in at any time the plaintiff can have the last say on that new matter.

The Court: No person will be prevented from presenting any fact that will have a bearing upon the elucidation of the issue here.

(PLAINTIFF'S REPLYING DEPOSITIONS.) 8869

Depositions to be read at the trial of this cause taken on behalf of the plaintiff under order of this court and pursuant to the United States statutes, before George C. Arvedson, Notary Public, at the Cruft High Tension Laboratory, Harvard College, Cambridge, Massachusetts, beginning July 5, 1916, at 2 P. M.

Met pursuant to notice.

Present: L. F. H. Betts, Esq., and
John W. Peters, Esq., Counsel for Plaintiff.
Philip Farnsworth, Esq., Counsel for Defendant. 8870

By Mr. Peters: I read in evidence the notice under which these depositions are taken, the same being as follows:

8871

8872 # UNITED STATES DISTRICT COURT,

WESTERN DISTRICT OF WASHINGTON,

NORTHERN DIVISION.

MARCONI WIRELESS TELEGRAPH COMPANY OF AMERICA,
Palintiff,

vs.

8873 KILBOURNE & CLARK MANUFACTURING COMPANY,
Defendant.

In Equity No. 71.

(NOTICE OF TAKING DEPOSITIONS.)

PLEASE TAKE NOTICE that, at 10 o'clock in the forenoon of July 3, 1916, at the Cruft High Tension Electrical Laboratory of Harvard University, in Cambridge, Massachusetts, we shall make certain tests of defendant's transmitter known as the "Simpson Mercury Valve Transmitter".

8874 You and the defendant's experts are invited to be present and to witness said tests.

ALSO PLEASE TAKE NOTICE that at the same place, at 10 o'clock in the forenoon of July 5, 1916, we shall take evidence and testimony on the part of the plaintiff based upon said tests, before William Nelson, Clerk of the U. S. District Court for the District of Massachusetts, or his deputy, or George C. Arvedson, a notary public, or other proper officer, pursuant to the order entered herein June 1, 1916.

Notice of Taking Depositions. 8875

The names and residences of the witnesses to be examined are stated below.

You are invited to attend and cross-examine any witnesses produced.

Yours, etc.,
SHEFFIELD & BETTS,
Plaintiff's Solicitors,
52 William Street,
New York City.

Names of Witnesses	Residence
Dr. E. L. Chaffee	Cambridge, Massachusetts
Prof. Charles R. Cross	Cambridge, Massachusetts
Dr. Louis W. Austin	Washington, D. C.
Prof. J. H. Morcroft	New York, N. Y.
Dr. J. G. Coffin	New York, N. Y.

8876

Dated New York, June 23, 1916.

To
PHILIP FARNSWORTH, ESQ.,
Counsel for Defendant,
149 Broadway,
New York City.

8877

Service admitted this 23 day of June, 1916.

(Signed) PHILIP FARNSWORTH,
Counsel for Defendant.

8878 (STATEMENT OF COUNSEL.)

By Mr. Peters: At the conclusion of Dr. Chaffee's test yesterday, I made the following offer to defendant's counsel and representatives:

To Counsel and Representatives of Kilbourne & Clark Manufacturing Company.

Gentlemen:

Dr. Chaffee has made certain tests today, which you have witnessed, in order to determine and to show to the court the true mode of operation of the Kilbourne & Clark apparatus. He believes and we shall represent to the Court that the apparatus as tested was in normal operative condition and ad-
8879 justment. If you have any reason to believe otherwise we now ask you to put the apparatus in what you consider proper condition and adjustment and Dr. Chaffee will repeat each and all of the tests. He will give you every opportunity tomorrow to do this and if you decline we shall contend that you should not be heard to criticize these tests as to these matters.

Yours truly,

John W. Peters,
Counsel for the Plaintiff.

And ask defendant's counsel what reply, if any, he desires to make to this offer.

8880 By Mr. Farnsworth: The question seems gratuitous in view of the fact that this morning at 9 o'clock I read over the telephone to Mr. Peters the reply which follows below, and sent by messenger a typed copy thereof to Messrs. Betts and Peters at their hotel, the Parker House, at about 11 o'clock A. M.

"To
L. F. H. Betts, Esq.,
and
John W. Peters, Esq.,
Counsel for Plaintiff.

"Sirs: At 8.30 P. M. yesterday (July 4th) at the end of two days of the so-called Chaffee tests,

 8881

made and completed without any request for co-operation by Defendant, we were informed that Plaintiff would 'represent to the Court that the apparatus as tested was in normal operative condition and adjustment.'' We then were notified also that plaintiff would contend that we ''should not be heard to criticize his'' (Chaffee's) ''tests'' unless defendant now put the apparatus in what it considers proper condition and adjustment.

The Plaintiff's said notice was orally given us and reads as follows:

''To Counsel and Representatives of Kilbourne & Clark Mfg. Co. 8882

Gentlemen:

Dr. Chaffee has made certain tests today, which you have witnessed, in order to determine and to show to the Court the true mode of operation of the Kilbourne & Clark apparatus. He believes and we shall represent to the Court that the apparatus as tested was in normal operative condition and adjustment. If you have any reason to believe otherwise we now ask you to put the apparatus in what you consider proper condition and adjustment and Dr. Chaffee will repeat each and all of the tests. He will give you every opportunity tomorrow to do this and if you decline we shall contend that you should not be heard to criti- 8883
cize his tests as to these matters.

Yours, etc.,

John W. Peters,
Counsel for the Plaintiff.''

''Defendant does not accept any of the responsibilities attempted to be put on it in the above notice orally given, for the reason that the Plaintiff long since refused Defendant's offer to make tests *inter partes*, so that the result is that Mr. Simpson, coming three thousand (3000) miles to these *ex parte* Chaffee tests, has come unprovided with the

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means necessary to put the apparatus into normal operating condition.

"Not only have these so-called Chaffee tests been conducted *ex parte* and with the apparatus in far from normal operating condition, but the results, even under such conditions, have failed to disprove the impulse converting trigger action of the Simpson Mercury Valve Transmitter.

"While we assume no obligation in the premises, yet in continuation of good faith, we will agree to have Mr. Simpson himself put this transmitter in normal operating condition, if now humanly possible in view of the Plaintiff's misuse of
8885 the same, and this he will do in the Cruft High Tension Laboratory of Harvard University at Cambridge, Mass., on or before July 8th next, provided:—

"First, that there be no further delay beyond July 8th of this cause which already has been so greatly delayed by Plaintiff, and provided

"Second, that Plaintiff will furnish Mr. Simpson with the means necessary to put the apparatus in normal operating condition, i. e., those things which Defendant has stood ready at all times to furnish to Plaintiff, but which Plaintiff has never requested of Defendant, and which Plaintiff has put it out of Defendant's own power to furnish at
8886 this place and late date, and which thing are necessary to enable Mr. Simpson to attempt to put the apparatus into normal operating condition, viz:

"(a) One 500-cycle generator of standard Defendant's construction, especially designed and manufactured for use with the Simpson Mercury Valve Transmitter, the same to be used in any further tests in lieu of the Marconi Company's generator used heretofore in the so-called Chaffee tests and marked "Marconi Wireless Telegraph Company of America, Serial Number 153.141, Patented March 12, and May 7, 1901", the same being

 8887

apparently especially designed and manufactured for use with the Plaintiff's transmitter.

"(b) A complete set of Simpson spark-gaps in normal condition as manufactured by Defendant, and designed especially for use in the Simpson Mercury Valve Transmitter, the same to be used in any further tests in lieu of the spark-gaps furnished Plaintiff some months since with the transmitter itself and used heretofore in the so-called Chaffee tests, which latter spark-gaps we expected would show damage due to Plaintiff's misuse of the apparatus, and which, in fact, Plaintiff has admitted on the morning of July 4th after the first day's tests to be so damaged, and which spark- 8888
gaps on July 4th for the purpose of the second day's tests were reconstructed under the supervision of Plaintiff's experts, without asking the advice or consent of Defendant's representative.

"We make no conditions or provisos other than the above but request the following, i. e., that Plaintiff will conduct the further tests under methods different from those heretofore employed in the so-called Chaffee tests in the following particulars:

"(a) Make photographs of every condition visible in the Braun Tube, instead of limiting the photography to only those special cases which seem to Plaintiff's experts to be favorable to their 8889
contentions.

"(b) Make photographs of only such conditions as occur when the apparatus is in ordinary commercial use, instead of photographing many and various special conditions which never arise in practice.

"(c) Make photographs at times only when the apparatus is producing a clear musical note, as normally, instead of photographing when it has been so put out of normal condition as to produce a poor sound or noise, as has been done heretofore in the so-called Chaffee tests.

8890 "(d) Make photographs only when the apparatus is connected, as normally, to a radio antenna, instead of using a 'dummy' antenna as has been done heretofore in the so-called Chaffee tests. We understand that two antannae are located on the Cruft laboratory.

"If Plaintiff should decline to accept Defendant's above offer of assistance, Defendant will state to the Court its belief that the Plaintiff's conduct of these so-called Chaffee 'Tests' is such as to be entirely consistent with Plaintiff's efforts throughout the trial of this cause to avoid and confuse the issue of the Simpson Mercury Valve

8891 Transmitter.

Very truly yours,

Philip Farnsworth,

Counsel for Defendant."

Boston, Mass., July 5, 1916.

Whereupon Dr. Emory L. Chaffee, a witness called on behalf of the plaintiff, being first duly sworn, testifies as follows:

Questions by Mr. Peters:

8892 Q. Please state your name, age, residence and occupation? A. Emory Leon Chaffee; age, 30; residence, 20 Highland Road, Belmont, Massachusetts; occupation, instructor.

Q. What experience and training have you had which qualifies you to operate and test radio telegraph apparatus and to operate and conduct tests with Braun tubes? A. In 1907 I received a degree of S. B. from the Massachusetts Institute of Technology in electrical engineering. I received a degree of A. M. from Harvard University in 1908, and a degree of Ph. D. from the same University in 1911. Since 1908 I have been engaged in instruction in Harvard University in courses of physics and electrical engineering, among which have been several courses in wireless telegraph and electrical oscillations. I have conducted several researches in the field of electrical oscillations in which I have made use of the Braun tube as a means of obtaining evidences or of obtaining the results. I am a Fellow of the American Academy of Arts and Sciences and a member of the French Academy of Arts and Sciences.

Q. What is your relation to the Cruft High Tension Laboratory? A. The Cruft High Tension Laboratory is 8894
simply an annex of the Physics Department. I am a member of the faculty and a member of the Physics Division.

Q. What particular relation has the Cruft Laboratory to Harvard University? A. The Cruft Laboratory is one of the physical laboratories of the University.

Q. What particular subjects or arts does it deal with? A. The Cruft High Tension Laboratory was built primarily for investigation in high tension work and in work concerned with electrical oscillations. Very little undergraduate instruction is given in the Laboratory, as I have said before, the Laboratory being primarily for investigation. 8895

Q. Do I understand that high tension electrical machines made by different manufacturers are investigated and tested at the Cruft Laboratory? A. Yes, to a certain extent that has been true. The tests have been made on several types of transmitting apparatus, purely from a scientific point of view.

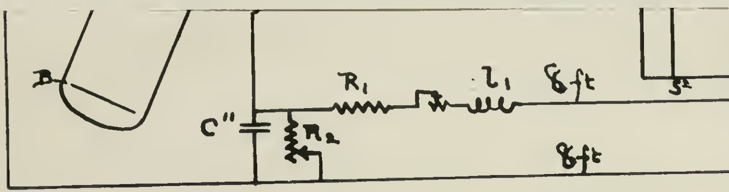
Q. When you refer to transmitting apparatus I presume you mean wireless transmitting apparatus? A. I mean wireless transmitting apparatus.

Q. Have you recently examined and tested a transmitting apparatus known as the Simpson Mercury Valve

8896

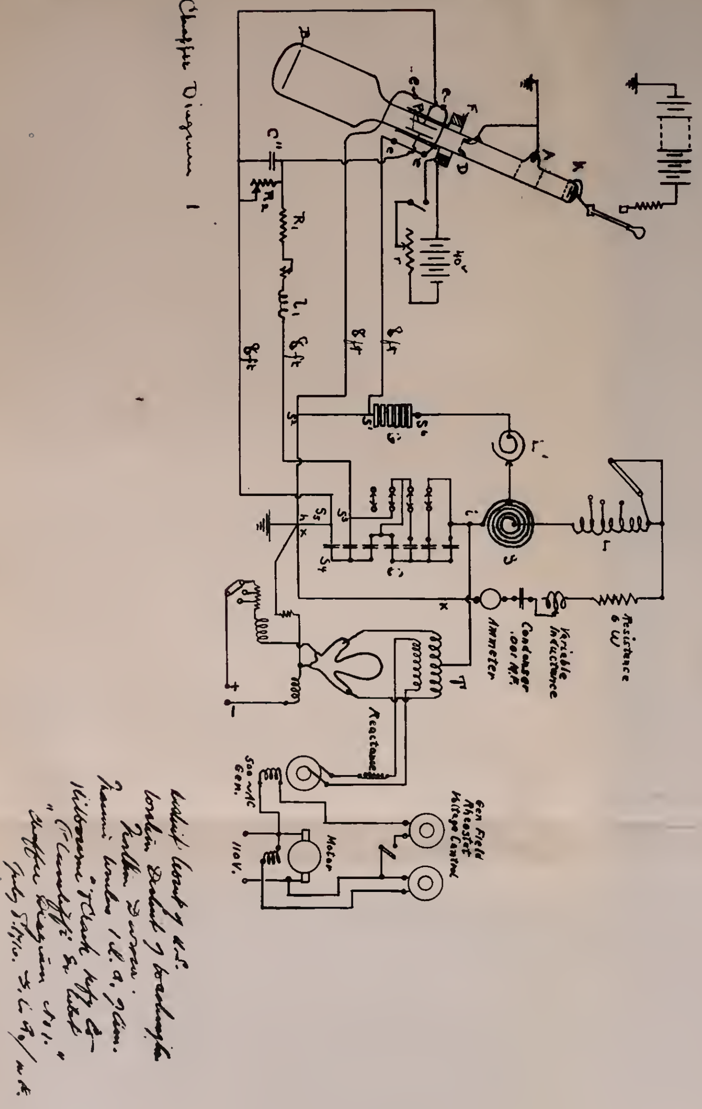
Transmitter of the defendant? If so, please state briefly the circumstances and in a very general way outline what you have done. A. The Marconi Company has asked me to make a test upon the said Simpson Mercury Valve Transmitter. The transmitter was sent to me previous to June 17th. I began active work upon the testing of the transmitter about June 17th and have conducted tests practically without intermission from that date to the present time. The transmitter was assembled and set up by Mr. Shoemaker of the Marconi Company. Before beginning work I operated the transmitter and re-tuned it
8897 so that in the tests which I am going to describe unless I say to the contrary I consider the set to be in operating adjustment. It is so adjusted as to give maximum radiation of secondary oscillations and good tone.

Q. Will you briefly describe by reference to a drawing or otherwise the arrangement of the Simpson Mercury Valve Transmitter which you tested? A. The connections of the Simpson transmitter I have carefully traced out and are as shown in the diagram which I produce and which I mark "Chaffee Diagram 1" (reproduced opposite). The transmitter consists of a large condenser denoted by C, connected in series
8898 with a spiral of copper ribbon S and an helix L. These elements are a part of the so-called open or antenna circuit, the antenna being connected to the free end of L and the ground being connected to the free end of the capacity C. The closed circuit is made up of the capacity C, about three-quarters of a turn or thereabouts of the spiral inductance S, a small separate inductance L′ and a spark-gap made up of five sections, denoted by G. The condenser C is charged through a mercury valve from the transformer T. This transformer is connected to a 500-cycle 125 volt generator. In



Chaffee Diagram 1

Plaintiff's Exhibit No. 67a.



2969

PEx 67a

-wing two complete
oscillations

→ time.

place of the usual antenna the tests which will be described were made with a dummy antenna. This dummy antenna consisted of a condenser of .001 micro-farads, a resistance of 6 ohms and an inductance, the value of which was adjusted so that the natural period of the dummy antenna taken alone would approximate to what is found in actual practice. The other connections shown on the diagram will be described later.

Q. You have described a closed circuit forming a part of the transmitter made up of spark gap G, variable inductants L′, a part of inductance coil S, and a condenser C. What can you say as to whether or not that 8912
closed circuit in the Simpson transmitter is an oscillatory circuit when the apparatus is in normal operation? A. I have made many tests in an endeavor to determine whether or not this closed circuit is oscillatory, and I find that there are always at least two oscillations in this closed circuit. I have observed two and one-half, and there may be more, but because of the nature of the test and the limitations of the apparatus, it is difficult to ascertain just how many there may be.

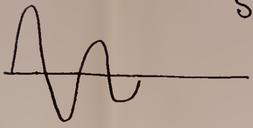
Q. You referred to two oscillations, or two and one-half oscillations in your last answer. Did you mean complete oscillations or incomplete oscillations? A. I mean 8913
complete oscillations, making four or five half oscillations.

Q. Will you make a diagrammatic drawing showing what you mean by two and by two and one-half oscillations? A. I may represent what I mean by two diagrams which I will call Chaffee Diagrams 2 and 3 (reproduced opposite). In Chaffee Diagram 2 I have represented what I mean by two complete oscillations. In this diagram time is represented by distances in a horizontal direction and is indicated on the diagram by an arrow and the word "time.' Distances perpendicular to this direc-

Chaffee 2

Plf Ex 67a

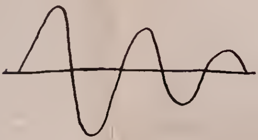
Showing two complete oscillations



→ time.

Chaffee diagram 3:

Showing two and one half complete oscillations



District Court of U. S.
Western District of Washington
Northern Division
Marconi Wireless Telegraph Co. of Am.
Kilbourne & Clark. Mfg. Co.
Plaintiff's Exhibit,
"Chaffee Diagrams Nos. 2 and 3"
July 5. 1916. S.C.A./N.R.

8914

tion represent intensity of current in the closed circuit. This I believe to be a familiar method of representing oscillations. I will mark this diagram by the words "Showing two complete oscillations." Chaffee Diagram 3 shows what I mean by two and one-half complete oscillations. I will mark this by the words "Showing two and one-half complete oscillations."

By Mr. Peters: I offer in evidence the tracing produced by the witness and ask the examiner to mark the same Plaintiff's Exhibit Chaffee Diagram 1. I also offer in evidence the single sheet of
8915 paper containing Chaffee Diagrams 2 and 3 referred to by the witness, and ask the examiner to mark the same Plaintiff's Exhibit Chaffee Diagrams 2 and 3. (Plaintiff's Ex. No. 67a.)

Q. When you tested defendant's apparatus and found the closed circuit to be oscillatory as above described did you ascertain whether or not the closed circuit was tuned to the open circuit or dummy antenna circuit? A. The two circuits have been tested as to their natural period and found to be within the error of measurements exactly in tune.

8916 Q. Please explain the nature of your tests and the observations on which you based your statement that the closed circuit was oscillatory in those tests. A. I made use of a Braun tube. The Braun tube which I used was approximately three feet long and inclined so that the direction of its length is coincident with the direction of the earth's magnetic field. The Braun tube in section is shown in Chaffee Diagram 1. It consists of the cathode or negative terminal K and the anode A sealed in the upper end, as shown on the diagram, of a glass tube of several centimeters diameter. The anode A is grounded. Cathode K is connected through a high water resistance

 8917

to the negative terminal of a storage battery, giving voltages up to 40,000. Most of the tests were made with from 30,000 to 35,000 volts impressed on the tube. Below the anode A distant about 25 centimeters from the anode is a metallic diaphragm in which there is a hole less than a millimeter in diameter. This metallic diaphragm I have denoted by the letter D. This metallic diaphragm is also connected to earth. About ten centimeters below this diaphragm are two plates sealed inside the tube opposite each other and about two centimeters apart. These I have denoted by the letter P. These plates will be used
for getting electrostatic deflections, as will be described 8918
later, and will be called "inside electrostatic deflecting plates." Below this point the bulb expands into a cylindrical enlargement at the lower end of which is a fluorescent screen B. Outside the tube and surrounding the electrostatic deflecting plates is an electrostatic shield made up of a single layer of fine wire wound on a pasteboard tube which is then cut its entire length. This shield serves to protect the interior of the tube from electrostatic fields caused by the outside current coils which I am now going to describe. About the tube at the point where the electrostatic plates are situated are two single
turns of wire, one on either side of the tube. These turns 8919
are shown in cross section by the small circles on the diagram which I have lettered c. The plane of these turns is parallel to the plane of the electrostatic plates. The current which passes around these turns causes a magnetic field, which in turn causes a deflection of the spot of light as seen on the fluorescent screen in a direction parallel to the planes of the coils. The electrostatic deflecting plates cause a deflection of the spot in a direction at right angles to the deflection caused by the current coils. Around the tube a small distance below the diaphragm D is a coil of wire denoted by F, which is connected to a

8920 battery of about 40 volts through a variable resistance r. This coil serves to make the fluorescent spot smaller and more brilliant. In the tests in which I have shown that the closed circuit of the transmitter is oscillatory, I have made use of the current coils previously described. The two wires leading from the current coils were connected to the points S^1 and S^2, lead S^1 S^2 of the closed circuit being the small braided lead which passes from the end of the spark gap to the ground and condenser C. The length of the lead between S^1 and S^2 is not over two inches.

8921 It will be seen that the adjustments of the transmitter cannot be altered by shunting about this short lead a current coil having many times its inductance and many times its resistance. It was thought in making these tests that a current deflection would be the only satisfactory means of telling what occurs in the closed circuit of the transmitter.

Q. Do you base the last statement on theory, or on your experience? A. I base the last statement both on theory and experience.

Q. You have described the general arrangement? A. Yes.

Q. Please continue your answer. A. Before showing
8922 the photographs which are the result of the tests which I made in this connection, may I point out that a current in one direction, through the current coil e will cause a deflection of the spot in one direction from the zero position. A current in the opposite direction, through the current coil, will cause a deflection in the opposite direction. If, therefore, a deflection is observed on both sides of the zero, it is a conclusive proof that the current through the current coil alternates in direction.

Q. Now that you have described the apparatus employed, will you please describe what you observed on

2975

Plaintiff's Exhibit No. 67a.

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Plaintiffs' Exhibit
"Schaffer Diagrams Nos. 4, 5, & 6."
July 8. 1916. S. C. G./N. P.

 8929

the screen in the Braun tube during your tests? A. When the set was in normal operation, there appeared very clearly on the screen of the Braun tube a central bright spot, on either side of which was a line with two or more beads, or brighter spots. I can best illustrate this by a diagram which I will call "Chaffee Diagram 4."

The central bright spot was very intense, compared with the rest of the figure. This is what would be expected, because an easy calculation shows that the spot is moving, under the action of the current, in the closed circuit only about 1/1000 of the time. Consequently the
deflection due to the current is light or dim, compared 8930
with the central spot.

Q. I have asked what you saw. Have you told all that you saw? My question was general. A. The diagram which I have shown marked "Chaffee 4" (reproduced opposite with diagrams 5 & 6) shows what I saw the greater part of the time. Occasionally there were three beads on one side of the zero and two on the other, conforming with the diagram which I call "3."

The set was purposely thrown slightly out of tune several times, and the appearance on the screen changed, the number of spots increasing in number.

Q. Is that the end of that answer? A. Yes, I have 8931
described what I saw on the screen.

Q. My question was a general one, and inquired what you saw when the Braun tube was connected in the various ways referred to by you, and shown in your drawing, Diagram 1. A. I have only described what I saw on the screen when connected according to my first test, in which only the current coils were in use.

Q. Will you please describe what you saw in the other tests? A. To confirm this test and to obtain additional evidence of the oscillatory nature of the closed or primary circuit, I made use of electrostatic deflections of the spot

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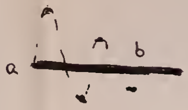
Chaffee 4

Chaffee 5



Chaffee 6

8932

in a direction perpendicular to the current deflections. This test was an endeavor actually to obtain a figure similar to the diagrams marked "Chaffee 2 and 3." The electrostatic plates "P" were connected across the terminals of a condenser "C2 of .002 micro-farads. Across the terminals of this condenser was connected a leak resistance, R2."

The condenser, "C2" was connected through a high resistance, "R1" to inductance, "1-1" and the points "S3-S4" shown in the diagram. This connects the condenser, "C2" together with the resistance, "R1 and in-
8933 ductance 2, 1-1" across two sections of the condenser "C."

I made a mistake in the points across which this condenser was connected. The condenser "C2", including resistance "R1" and inductance "1-1" was connected across points "S3" and "S5", instead of across "S3" and "S4."

In some of the tests this circuit was connected to points "S5" and "S4" but never to points "S3" and "S4."

The inductance "1-1" consists of 100 or 200 turns on a small iron core. "R1" was of the order of 9,000
8934 ohms. "R2" was usually about 2,400 ohms.

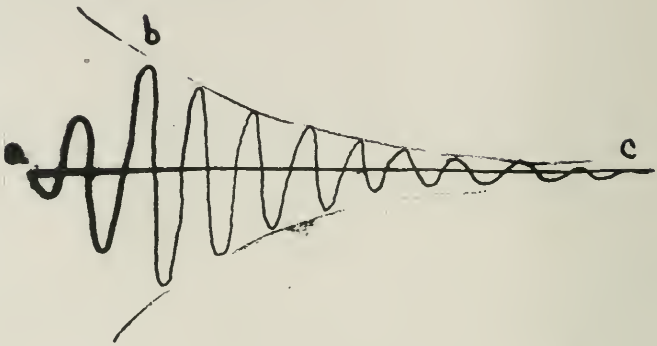
This circuit serves to give a time axis to the deflection in the Braun tube. When both the current coils "e" and the static deflecting plates "P" were used, the appearance on the screen was as shown in Diagram 5. This diagram shows the oscillations spread out into a wave train, similar to Diagrams 2 and 3.

As I have stated before, at times it was possible to see the evidence of five half loops in the primary circuit. The diagram corresponding to that case is shown in Diagram 6.

2979

tiff's Exhibit No. 67a.

Chaffee 9.

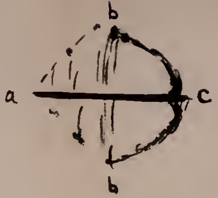


By Mr. Peters: I offer in evidence the single sheet produced by the witness, containing his diagrams 4, 5 and 6 and ask the Examiner to mark the same "Plaintiff's Exhibit, Chaffee Diagrams 4, 5 and 6." (Plaintiff's Ex. 67a.)

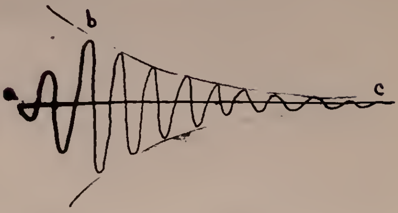
Q. Did you make any observations by means of the Braun tube of the oscillations in the antenna circuit, of defendant's apparatus? If so, will you please describe them briefly and illustrate them as you have done in the case of the other observations? A. The oscillations in
the antenna circuit were investigated in the same manner 8942
as just described, the difference being that the current coils "e" were connected with the antenna circuit, instead of with the closed circuit. The terminals of the current coil "e" were connected to two points on the lead, leading from the ammeter shown in the diagram to the ground. In other words, the current coil may be said to be connected to points "X-X", the distance between those two points being about two feet. In this case, the spot will be deflected by the current in the antenna circuit, and perpendicular to this deflection by the static plates "P." The figure seen on the screen was
approximately as shown in Diagram 7 (repro- 8943
duced opposite with diagrams 8 & 9). The portion of the figure between "b" and "c" represents the dying down of the oscillations of the antenna. The peculiar shape at the end of the diagram at "c" is due to the fact that the spot starts to move back before the oscillations in the antenna circuit are practically zero in amplitude. The portion between "a" and "b" represents the building up of the oscillations of the antenna, and represents the same interval of time as was shown on the diagrams 5 and 6, between points "a" and "b." The fact that the

Plaintiff's Exhibit No. 67a.

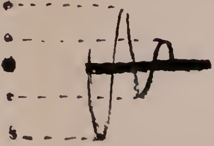
Chaffee 7



Chaffee 9.



Chaffee 8



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"Plaintiff's Exhibit
"Chaffee Diagrams Nos. 7. 8. and 9"
G.C.G.
N.P.
July 6. 1916

8944

amplitude of the oscillations in the secondary requires several complete oscillations before they attain their maximum value, as shown at "b", conclusively proves that the energy was being received during this interval from the primary circuit, which further proves that the primary circuit must have been oscillating during this time.

Q. Can you produce photographs, made in the presence of defendant's representatives, showing graphically the oscillations in the primary and secondary of defendant's apparatus, such as you have illustrated in your dia-
8945 grams 4 to 7 inclusive? A. I have neglected to say how these photographs were taken. The camera was supported above the screen "B" of the tube in such a position that practically a natural size photograph was taken of the figure as shown on the screen "B". Photographs "B-A" and "B-B" were taken for wave one of 595 metres and show what I have attempted to draw in Diagram 4. The two plates were taken under the same conditions. There are two exposures on each plate. The top (which is the figure nearest the inscription on the plate) is for a condition of one discharge of the condenser "C" through the primary circuit per half cycle of charging
8946 current. Five gaps were in operation.

The figures at the bottom of the plate were taken with two discharges of the condenser C through the primary circuit per half cycle of charging current when three gaps were in operation, the other two having been short circuited by clips provided for the purpose.

The Witness: The brightness of the central spot in comparison with the straight line deflection should be noticed in these pictures, because, as I have already pointed out, the spot of light is moving and giving the straight line impression only

 8947

about one one-thousandth of the time. Plates BC, BD, BE and BF are photographs showing the oscillations in the primary circuit when the electrostatic plates P of the first diagram are connected and correspond with what I have endeavored to draw in diagram 5. In these photographs the very weak straight line deflection of the previous photographs has been still further spread out and is consequently correspondingly weaker. It is, therefore, more difficult to get a sharply defined figure in this case than in the former case. Nevertheless,
there can be seen on the pictures evidence of oscil- 8948
lations, proving the existence certainly of two complete oscillations and in one or two cases showing strong evidence of probably more. Plate BC was taken for 595 meters and two discharges of the condenser C through the primary circuit per half cycle of charging current. Plate BD was the same as BC, except the set was adjusted to operate at one discharge of the condenser C through the primary circuit per half cycle of charging circuit. Plate BE was taken under the same conditions as was plate BD. Plate BF is a duplicate or a repe-
tition of the conditions for plate BC. Plates BJ, 8949
BK, BL and BM were taken under the conditions described for showing Diagram 7. BJ is for the condition of one discharge of the condenser C through the primary circuit per one half cycle of charging current with 5 gaps operating. The static deflecting system, which gives the time axis to the figure, was the same as was used in plates BC, BD, BE and BF. Plate BK was the same as BJ except there were two discharges of the condenser C per half cycle of charging current and

three gaps were in operation, the other two having been short circuited by means provided. Plate BL is the same as BK except for a slight change or readjustment of the static deflecting system. Plate BM is the same as BL except for a further readjustment of the static deflecting system. I have offered all the plates taken at the tests. Some of them are clearer than others, the success of the exposure depending, of course, upon the regularity of the operation of the set during the time of exposure. Plate BH is an attempt to combine the
8951 single current straight line deflection as represented in plates BA and BB with the combined electrostatic and electromagnetic deflections, as shown in plates BC through BF.

Q. What are the two images on the last plate referred to by you? A. The image at the top of the plate nearest the inscription represents a deflection of the spot caused by the current in the primary or closed circuit and shows the existence of oscillations. By the addition of the electrostatic deflecting plates this straight line deflection is developed out into a wave train, as shown in the second
8952 image of the same plate. The beads on the straight line deflection at the top of the plate may be projected horizontally and will be seen to correspond with the impressions representing the maximum amplitude of the oscillations, as shown in the wave train. This I might further represent by a drawing, which I will call Chaffee 8. This figure serves to illustrate the connection between the photographs BB and the figures obtained in photographs BC through BF.

Q. What was the time period, speed or frequency of the oscillations which you have photographed on the

 8953

plates produced by you? A. The wave length of the oscillations in the antenna circuit was 595 meters, or approximately 600 meters. The time of one complete oscillation was therefore one five-hundred-thousandth of a second. The time of one complete oscillation in the primary or closed circuit is the same, and therefore one-five-hundred-thousandth of a second. If there are two complete oscillations in the primary circuit, the spot of light moved over the trace showing the primary wave train in pictures BC through BF in one two-hundred and fifty-thousandth of a second. Of course, the spot of light re-traced this journey a thousand times each second if 8954
the condenser C is discharging once per half cycle of charging current or two thousand times if discharging twice per half cycle of charging current. The exposures for these photographs ranged from three to ten seconds. It is, therefore, clear that the spot of light re-traced the figure from three thousand to twenty thousand times during the exposure. Any irregularity in the operation of the set can very easily cause a blurring of the photograph, which explains why the definition of some of the photographs is poor.

It is stipulated that the photographs referred to in the preceding answer were made in the presence of the 8955
following gentlemen:

Messrs. Chaffee, Cross, Morcroft, Coffin, Peters, Weagant and Waterman, representing the plaintiff,

Messrs. Farnsworth, Simpson, Stone, Pickard, Zenneck, Simon, Kolster, representing the defendant.

By Mr. Farnsworth: The above stipulation is made without any admission as to the competency or materiality of these photographs, not the slightest evidence of which has yet been adduced.

By Mr. Peters: I offer in evidence the eleven photographs (negative plates) produced and referred to by the witness, marked—BA, BB, BC, BD, BE, BF, BJ, BK, BL, BM, BH, and request the Examiner to mark the same collectively "Plaintiff's Exhibit Chaffee Photographs, BA, BB, BC, BD, BE, BF, BJ, BK, BL, BM, BH.

What can you say as to whether or not these eleven
8957 photographs set forth a fair representation of what you saw on the fluorescent screen during the particular tests in which they were taken? A. These photographs are an exact representation of what was on the screen. It is very often true that the figure as seen on the screen is clearer and more distinct than the photographs. This is because the eye records an image in a short time when the regularity of the set is such as to give a clear figure, but the length of the exposure is long and the plate records all the irregularities which occur during the exposure.

Q. What can you say from your observation during
8958 the tests and from the photographs as to whether or not defendant's transmitter operates by impact excitation? A. I have had the set under test for two weeks and a half. I have made many adjustments of the several variable parts of the set. I have taken over one hundred and fifty photographs and in no case have I seen less than two complete oscillations.

Q. I do not quite understand you. What is the significance of the two oscillations which you say you have always seen? A. By impact excitation is only meant a discharge in the primary circuit consisting of a single

 8959

half loop, or to put it in another way, by a discharge which does not reverse in direction. If oscillations exist in a primary circuit consisting of from four to five half loops of current discharge, the set cannot be spoken of as an impact set.

Q. Please refer now to your diagram 7 and draw a conventional diagram showing the building up and decay of current which you say is represented in this diagram 7. Please draw this opposite Figure 7. A. I have represented in Figure 9 in a conventional way the building up, between points a and b, and the dying down between points b and c, of the secondary oscillations, which 8960
interprets the figure shown in diagram 7. Energy is being supplied to the secondary circuit from a to b, this supply of energy coming from the primary circuit. Between points b and c the energy which has been established in this secondary circuit is being dissipated in heat or radiation, or both. The secondary is oscillating freely between points b and c.

Q. In Diagram 7, where is the elongated tail of the chain shown in Figure 9 at the end "c"? A. Because of the nature of the system which gives the electro-static deflection the oscillations at the "c" end of the train represented in Figure 9 are compressed horizontally and 8961
even lap back a bit toward point "a". In other words, the spot of light in Figure 7 moves from "b" to "c", and a short distance back toward "a" during the time interval shown in Figure 9, between points "b" and "c."

Q. In Figure 9, is the time axis "a" to "c" intended to extend from one end of the chain to the other? A. Yes. I have corrected the diagram by extending the zero line, or straight line throughout the whole chain.

Q. What produces the halo surrounding the dots and lines on the eleven photographs which you have produced? A. The halo, or fogging of the plate around the central image is due partly to what is known as "halation," in photography, that is the fogging of the plate due to the reflection of light back and forth between the surfaces of the glass plate.

The fogging is also, in part, due to stray light, which is seen to surround the bright central image, even while the spot is not being deflected. This stray light is present when the vacuum in the Braun tube is adjusted for best working conditions, and is present because, in order to get a sufficiently brilliant spot, large currents are used in exciting the Braun tube.

8963 Q. Have you made any tests of defendant's transmitter to ascertain the action of the mercury rectifier on the oscillatory character of the closed circuit, and if so, what tests, and what did you ascertain to be the fact in this regard? A. I have made tests when the set was operating under normal conditions, and immediately thereafter with the mercury rectifier not operating, one side of the mercury rectifier having been short-circuited.

The operation of the set, so far as the oscillatory character of the primary current is concerned, is shown to be identical in the two cases. This is borne out by a photograph marked "B-G." The figure at the bottom
8964 of the plate, which is the end of the plate away from the inscription represents the single line current deflection, due to the primary current when the set is in normal operation, working with two discharges of the condenser "C" per half cycle of charging current. This figure is identical with the figure shown at the bottom of plate "B-A." The middle exposure on plate "B-G" shows the result when, everything else being left the same, one side of the mercury rectifier is short-circuited. In order that the discharges of the condenser "C" may occur the same number of times per half cycle of charging current, the field rheostat of the generator was slightly in-

 8965

creased. The two figures are identical, except the middle one shows slightly more blurring than the bottom exposure. This is what would be expected, because the condenser "C," when the mercury rectifier is not in operation, is charged in opposite directions each succeeding half cycle of charging current; whereas, when the rectifier is in operation, this condenser "C" is always charged in the same direction. Consequently, the figure shown at the bottom of plate "B-G," when the mercury rectifier is in operation, must be thought of as being alternately reversed to get the figure in the middle
of plate "B-G." Since the figure at the bottom of the 8966
plate "B-G" is not symmetrical about the central image, the super-position of this figure, first in one direction and then in the other direction to give the middle picture of plate "B-G" must necessarily cause a blurring of the small bead-like points which one sees in the lower figure.

The straight line deflection on either side of the zero point in both cases demonstrates the existence of oscillations.

The figure at the top, or nearest the inscription, is for the same condition as for the middle exposure, except that the set is adjusted for a discharge of condenser
"C" through the primary circuit once per half cycle. 8967

Plate "B-G" and other plates showing the same thing demonstrate that the addition or omission of the mercury rectifier causes practically no difference in the nature of the discharge through the primary circuit.

Q. Did you make any test to ascertain what effect, if any, is produced in defendant's apparatus by including the condensers in a circuit common to the closed circuit and to the radiating circuit? A. I made tests on the transmitting set to determine if a shifting of the lead "X-X" from the point "h" to the point "i" in Diagram 1 made any essential difference in the operation of the

transmitting set. If this lead be shifted from "h" to "i," a readjustment of the set is necessary, in order to obtain maximum radiation. It is not easy, with the range of adjustments at hand, to put the set in accurate tune when the lead is transferred from "h" to "i," but from the experiments which I performed, I believe that the operation of the set is essentially the same. This is partially borne out by a photograph on plate "B-T"; the bottom picture or exposure on the end of the plate away from the inscription shows the usual current straight line deflection, for the primary circuit, when
8969 the set is operating under normal conditions; adjusted for two discharges of condenser "C" through the primary circuit per half cycle of charging current, and is the same as the picture at the bottom of plate "B-A."

The exposure at the top was made with the same arrangement of Braun tube connections. When the lead "X-X" is transferred from "h" to "i," and after having readjusted the circuits for as good operation as could be obtained in the limited time. In both cases, oscillations are shown to be present in the primary circuit.

Q. As I understand you, the inclusion or omission of the mercury rectifier does not affect the oscillatory char-
8970 acter of the circuit. Did you note any material effect on the system when you inserted or removed this device? If so, please explain. A. When the mercury rectifier is in operation, both halves of the secondary, of the transformer "T" in Diagram 1 are in operation. When the rectifier is omitted, and one-half of it short-circuited, the full load of the set is borne by one-half of this transformer. It can hardly be expected that the operation of the transformer would be the same in the two cases. So far as the oscillatory character of the discharges in the primary circuit of the transmitting set is concerned,

I can see no difference. A very good tone was obtained
from the system when the mercury rectifier was omitted,
and essentially the same amount of radiation.

Q. I have also understood from your testimony that
the shifting of the condenser "C", so as to remove it
from the radiating circuit did not affect the oscillatory
character of the circuits. Did it have any material effect
otherwise on the system? Did this shifting have any
material effect otherwise on the system? A. The shift-
ing of the lead "X-X" from point "h" to point "i" did
cause a difference in the operation of the system, neces-
sitating some retuning, although the difference was slight. 8972

I believe that the set could be put in satisfactory
operation with the lead "X-X" connected to the point
"i", the difference being simply a change in coupling be-
tween the closed and open circuits, and a very slight
shifting in the wave length of the open circuit.

By Mr. Peters: I offer in evidence the single
sheet of paper produced by the witness containing
his diagrams 7, 8 and 9, and ask the examiner to
mark the same "Plaintiff's Exhibit Chaffee Dia-
gram 7, 8 and 9". (Plaintiff's Exhibit 67a.) I
also offer in evidence the two photographic plates 8973
last produced by the witness, identified as BG and
BT. The same are marked "Plaintiff's Exhibit
Chaffee Photographs BG and BT." (Pl. Ex. 67e.)

By Mr. Farnsworth: We will request to have
produced all of the photographs which have been
made in this connection, including all of those
made July 3rd and 4th, and all of those made in
the preliminary trials, and I state this now in or-
der that plaintiffs may have ample time to obtain
prints of all such negative photographs and give
them to us at the earliest moment. We particu-

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larly request that in the meanwhile all the photographic negatives be retained by Dr. Chaffee and the prints made under his supervision.

Adjourned to Thursday, July 6, 10 A. M.

CAMBRIDGE, MASS., July 6, 1916.

Met pursuant to adjournment.

8975 Present: Counsel as before.

DIRECT-EXAMINATION OF MR. CHAFFEE CONTINUED.

Q. Have you read the testimony given in this case by Messrs. Simpson, Kolster, and Greaves, regarding certain tests of defendant's apparatus, by means of a Braun tube? A. Yes, I have read the testimony of Messrs. Kolster, Greaves and Simpson.

Q. Were the methods employed in those tests proper methods for determining whether or not oscillations existed in the circuits? A. I consider the method employed
8976 in those tests unsuitable for proving the existence or non-existence of the oscillations in the closed circuit.

Q. Please state your reasons. A. In those tests, outside electrostatic deflecting plates were used. As I will point out, the results obtained when outside electrostatic deflecting plates are used are very difficult to interpret, and give effects which have no bearing upon the point in question; that is, give no evidence as to the existence or non-existence of oscillations in the closed circuit.

If inside electrostatic plates had been used, the results obtained would have been more easily interpreted, but

even although the arrangement of the Braun tube might have been proper, that is if inside electrostatic plates had been used, the method of connection to the apparatus I consider to be improper for demonstrating the existence or non-existence of oscillations. The question at hand is whether or not there is an oscillation of current in the closed circuit. The observations at those tests were made while using electrostatic deflections due to the potential across the gap. Assuming, then, that the Braun tube methods were correct, the result obtained when electrostatic deflections were made use of would be
poor evidence of the existence or non-existence of cur- 8978
rent oscillations. The potential across the gap depends upon the current flowing in the closed circuit, and upon the resistance of the gap, which is variable; and upon the potential of the condenser "C", when the closed circuit is not in operation. Since the closed circuit is carrying a current approximately 1/1000 of the time, the deflections which would be obtained in a Braun tube if properly connected to show the variation in potential across the gap, would be a picture primarily giving the variations in potential of the condenser "C" while it is being charged.

Going now to the arrangement of the Braun tube used 8979
in these tests, it will be remembered that outside electrostatic plates were used. A very simple test conducted on Monday, July 3, showed that the deflections obtained when using the insider plates gave a proper measure of the potential of those plates, and consequently of the potential between the points to which the plates were connected.

If the outside plates are used, the spot of light is deflected only when the potential is varying. Even although the potential across the outside plates may be as much as 20,000 volts, yet, if that potential be steady, the spot is undeflected. It is seen, then, that the outside plates give an entirely erroneous measure or idea of the actual potential which exists between the points to which the plates are connected.

I have obtained, on repeating the test above referred to; that is, the Washington test, a reat variety of figures, dependent entirely upon the adjustment of the field rheostat of the generator. I have been able to reproduce Figures 1 and 2 on Photographs "F, G, S 4" (See Vol. 2, p. 1102), by a simple variation in the field rheostat of
8981 the generator.

I have been able to produce Figure 1 of "F G. S. 4" with the antenna disconnected.

I have been able to reproduce Figure 2 with the antenna connected and in normal operation, but, as I have said, these figures do not prove or disprove the existence of oscillations in the closed circuit, but simply give, when interpreted properly, the way in which the primary condenser is charged.

This peculiar action of the outside electrostatic plates, which renders them unfit as a method of determining the potential across which they are connected, I can explain
8982 completely on theoretical grounds, but because of the technical nature of the explanation, I will not go into it unless it is asked for.

Q. Will you please produce and explain any photographs made by you on July 3 or 4, when repeating defendant's Washington University Braun tube tests, in the presence of defendant's representatives? A. In the tests which I have made to bear out these points, I did not use the inside electrostatic plates "P" of my Diagram 1, or the current coils "E". The coil of wire "F" was moved up, and outside electrostatic plates placed about the tube,

 8983

about an inch or an inch and a half below the diaphragm "D". During the tests, to show that the position of the outside plates made no difference, the plates were moved to a point below the current coils "e" just above the point where the tube expands into the large bowl, or cylindrical part.

These outside electrostatic plates were connected across the gap "G" at points "S1" and "S6."

Plate "B O" shows four exposures on the same plate, obtained by each time moving the lense a short distance parallel with the plate, in order to vary the position of
the image. The bottom figure, the figure furthest 8984
from the inscription on the plate, shows the position of the spot when the field rheostat was adjusted so low that no sparking occurred; yet the condenser "C" was charged to practically full potential; that is, a potential of the order of 2,000 volts.

The upper figure on the photograph, the one nearest the inscription, shows the natural zero, when there is zero potential across the outside electrostatic plates. The position of those two points bears out what I have previously said, that if the potential of the outside plates is constant, the spot is undeflected.

The second figure (counting from the bottom of the
plate) is a reproduction of Figure 1 of "F. G. S. 4" and 8985
shows the deflection of the spot when the gap is discharging less than the normal number of discharges per half cycle of charging current.

The third figure from the bottom shows the same characteristics as does Figure 2 of "F. G. S. 4" and was obtained by simply increasing the field rheostat of the generator, so that the gap was discharging about once per half cycle.

Plate "B S" shows the figure obtained by the same arrangement of Braun tube when the antenna is disconnected from the set at both ends.

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The figure nearest the inscription, or at the top of the plate, is a reproduction of Figure 1 of "F G S 4", which, it was said, proved that the primary circuit does not oscillate yet. I have a photograph which conclusively proves that the primary circuit does oscillate at least seven times when the antenna is disconnected.

The lower figure of plate "B S" was taken under the same conditions. It shows a small, circular bright spot about in the middle. This was due to the fact that in the midst of the exposure the sparking ceased for an instant, and the spot returned to its zero position.

8987 I may point here that the position of the zero point may be anything, practically, with reference to the figure; for if when the outside static plates are used a simple sinusoidal potential, or a potential which varies symmetrically from either side of zero be impressed on the plates, and one of the plates grounded, the whole figure is seen to move over, so that the natural zero point is not at the middle of the figure. Therefore, no conclusions can be drawn from the natural position of the zero with reference to any figure which may be obtained when using outside electrostatic plates.

Plate "A F" shows the figure obtained when the electrostatic plates are connected as described above; the set
8988 adjusted for normal operation, with the antenna on, and the ground on, but with one side of the rectifier short-circuited.

Plate "B R" the figure second from the bottom, or third from the inscription on the plate, shows the figure taken under the same conditions; that is, with the rectifier out, the antenna on, and the ground on; the only difference being that in "A F" the gap was sparking once per half cycle or charging current, whereas in the figure on "B R" three gaps were in operation, the other two having been short-circuited by means provided, and the gap was discharging once per half cycle, as in the case of "A F."

The figure on "B R" is a reproduction of Figure 3 on "F. G. S. 4", and is exactly what would be expected from theoretical considerations.

The figure on "A F" is slightly different, but shows that many conditions may serve to alter the figure obtained under these conditions.

Q. Then, as I understand your last answer, your tests show that the Washington University test, said to be shown on defendant's exhibit "F. G. S. 4" shows simply
the charging of the condenser "C" by the low frequency 8990
generator current, and does not show the absence of oscillations in the primary closed circuit during the discharge of the spark gap. Am I right? A. Yes. I have pointed out before that the deflection of the spot shows the low frequency charging of the condenser "C", together with some spurious effects due to the fact that outside plates are used. They do not show the existence or non-existence of oscillations in the closed circuit.

May I point out the brightness of the deflections obtained when the electrostatic plates are used, in contrast with the very dim deflections produced by the actual current deflections in the closed circuit, on the plates which 8991
I have previously referred to? I think it is important to bear in mind that the actual oscillations in the closed circuit are taking place, according to calculations, only about 1/1000 of the time, and in order to make visible the deflections due to this current, it was necessary to use very large currents for exciting the Braun tube. An electrostatic machine was tried as a source of excitation for the Braun tube, with no success, because of its insufficient power. Recourse was therefore made to the high voltage storage battery, to which I have referred. It can, therefore, hardly be supposed that the brilliant

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deflections so easily obtained by the electrostatic plates can represent current oscillations in the primary circuit.

Q. In your photograph "B R" I believe you have not referred to the two images at the top, and the small image at the bottom. Will you please do so? A. I did not refer to the other three images of plate "B R" because they are essentially the same as others which I have already used.

The image at the top of "B R" is the natural zero position of the spot. The second image from the top of "B R" shows the behavior of the spot when the rectifier is in operation, the antenna is off at both ends, and
8993 is a repetition of the condition shown on plate "B S". It was in the latter exposure that the operation of the set was irregular, and I think stopped for an instant. I therefore discarded this exposure.

The lower figure of "B R" represents what is seen when the set is in normal operation, one discharge of the gap per half cycle, three gaps being used, the other two having been short-circuited; the field rheostat of the generator having a slightly different adjustment than was the case for the figures of plate "B O". I have already said that practically anything can be obtained according to the variation of conditions. I have not referred to
8994 this lower figure of "B R", because it has no particular bearing.

Q. When employing the Braun tube as it was used by the defendant in the Washington tests, what effects are had by varying the field of the generator? A. I have pointed out that all the figures of plate "B O" are obtained by simply a variation of the field rheostat of the generator. The change from one figure to another, as the field rheostat is varied, is more or less continuous; consequently a great variety of figures can be obtained by this simple variation of the field rheostat.

Q. What effect does the variation of the field rheostat have on the charging of condenser "C"? A. The variation of the field rheostat probably alters the point on the charging potential wave, at which the gap discharges. It also alters the rate at which the condenser "C" will be charged, after the condenser "C" has discharged through the primary circuit. It is, then, a difference in rate of charging and a difference in point of the charging cycle at which the gap discharges, which gives the changes in the figures observed.

Q. As I understand you, the variation of the field rheostat varies the voltage of the current supplied to the
condenser? A. Yes, the variation of the field rheostat 8996
does vary the voltage of the charging transformer, which causes the effects which I have described.

> By Mr. Peters: I offer in evidence the four photographic negative plates last referred to by the witness. The same are marked "Plaintiff's Exhibit Chaffee Photographs B O, B S, A F and B R." (Plaintiff's Exhibit No. 67e.)

Q. As I understand you, all of these photographs which you have so far produced were made either on July 3 or July 4, in the presence of defendant's repre-
sentatives; is that true? A. That is true. 8997

Q. Have you any other photographs made on July 3 or 4, in the presence of defendant's representatives? If so, will you please produce them? A. Thirty-three photographs were taken on July 3, thirty-two of which are here; one of which has already been offered in evidence as "Chaffee Photograph A F."

The tests made July 3 are denoted by single letters of the alphabet, and by double letters beginning with "A"; the photographs taken July 4 are similarly denoted with a "B", as a prefix to the inscription.

Q. Will you please refer to any of the photographs of July 3 which you have just produced, which you think it desirable to refer to, in order to further explain the series of tests made in the presence of defendant's representatives? A. I don't care to refer to any of the photographs of the tests of July 3, other than the one marked "A F," which I have already referred to, because the set was operating irregularly, and the photographs do not show, as clearly the results which I wish to bring out as do the photographs of July 4.

There are two photographs I might refer to, in showing
8999 ing that the operation of the set July 3 was essentially the same as the operation July 4, with the exception of irregularities due to the poor condition of the set on July 3. Plate "Y" shows the current straight line deflection, due to the current in the primary circuit when the set was adjusted for 595 metres, and the gap was discharging twice per half cycle of charging current, and shows the same characteristics as plates "B A" and "B B", of the tests of July 4.

Plate "Q" was taken when the current coils "E" of diagram 1 were connected in the usual way in the primary circuit, and the electrostatic deflecting system used
9000 in plates "B C", "B D", "B E", and "B F" was connected. The combination of the electro-magnetic and electrostatic deflections gave the wave train. The figure shows the oscillations of the primary circuit when the antenna circuit was disconnected at both ends.

By Mr. Peters: I offer in evidence the two photographic plates last referred to by the witness, and the same are marked "Chaffee Photographs Q and Y."

Q. Please now produce all of the plates made on July 3 and 4 in the presence of defendant's representatives which have not been produced and offered in evidence, in order that I may comply with the request of defendant's counsel. A. I have done so. There are four remaining from the tests of July 4, and thirty of the series taken July 3.

By Mr. Peters: I now offer all the plates produced by the witness to defendant's counsel.

By Mr. Farnsworth: Does plaintiff offer these 34 photographic negatives in evidence?

By Mr. Peters: Plaintiff does not consider it necessary to do so.

By Mr. Farnsworth: Defendant does. 9002

By Mr. Betts: Then defendant can offer them.

By Mr. Farnsworth: Defendant does.

By Mr. Peters: Have them marked as defendant's exhibits.

By Mr. Farnsworth: I don't care what they are marked. Defendant offers in evidence the 34 photographs.

Q. You have stated that the apparatus was not working regularly during the tests of July 3. Will you please explain more fully regarding this. A. The set on July 3 operated very irregularly, it being impossible to obtain a clear note and the irregularities served to blur the 9003
figures obtained in the Braun tube. Furthermore, the day was very humid and there was considerable leakage from the 35,000 volt line brought from another laboratory 200 feet away, and this leakage caused irregularities in the operation of the Braun tube.

After the series of tests had been finished July 3 it was suspected that the gaps were in poor condition. One was opened and found to be badly burned and pitted. The others were tested before being opened and two were found to be short-circuited internally. The four remaining gaps which had not been opened were then opened.

Two were found to be in fair condition. The other two, which were the two which had been found to be short-circuited, were very badly burned and pitted. The gaps were carefully machined, the sparking surfaces put in good condition, the gaps sealed air tight by sealing wax and put together so that the normal spacing existed between the sparking surfaces. The tests of July 4 were therefore made with the repaired gaps.

After the plates BA, BB, BC and BD were taken all gaps were opened in the presence of the defendants and the gaps were found to be in good condition, showing no
9005 abnormal pitting or burning.

Q. Did you make any inspection of defendant's spark gap to ascertain whether the coloring and pitting was uniform on all the terminals, as would be caused by oscillatory current passing between the terminals? A. I examined the gaps carefully and could see no unsymmetry in the appearance of the two terminals after having been opened on July 4.

Q. Describe the condition of the gaps when you opened them on July 3 in this regard. A. The coloring of the two surfaces of the gaps was practically identical. At the points where the gap was badly burned there was a raised part on one plate which corresponded with a hollow
9006 in the other plate. I am not sure whether the raised part occurred always on the same side of the gaps.

It is stipulated that all the photographs heretofore produced and referred to by the witness made on July 3 and 4 were made in the presence of defendants representatives as heretofore stated, and that in addition, on July 3, W. J. Barclay and J. A. Proctor were present, representing the defendant.

By Mr. Farnsworth: The above stipulation is made without admitting the competency of the photographs, not in respect to Dr. Chaffee's testimony, but the competency in respect of what defendant believes plaintiff's witness Mr. Waterman will endeavor to represent the photographs prove.

Q. Did you individually take all of the photographs which you have produced and referred to? A. I took all of the photographs individually.

Q. And they were all marked at the time they were taken by Mr. Simpson of the defendant company? A. That is true.

Q. In view of defendant's request that all photographs made by you on the Simpson transmitter be pro- 9008
duced, I will now ask you to produce each and every photograph made by you during your tests, which I understand commenced more than two weeks ago, not including those of course that have already been produced. A. I have already produced all the photographs made on July 3, and 4. I now produce 110 photographs taken previous to July 3, and have carefully checked them up with my records in the notebook.

By Mr. Farnsworth: Defendant gives notice pursuant to the stipulation of May 31, 1916, and the order of June 1, 1916, that beginning at ten
A. M. on Monday, July 10, next, at the offices of 9009
Philip Farnsworth, Esq., 149 Broadway, New York City, defendant will take the testimony of witnesses on its behalf, before A. Z. Brown, a Notary Public, or other proper officer.

The names and residences of the witnesses to be examined are as follows:

Dr. Jonathan Zenneck, Munich, Germany,
Prof. John Stone Stone, New York City,
Mr. F. A. Kolster, Washington, D. C.,

9010 Dr. Emory L. Chaffee—Direct.

Mr. Greenleaf Whittier Pickard, Amesbury, Mass.,
Mr. Emil Simon, New York City, and
Mr. Frederick G. Simpson, Seattle, Washington.

Inasmuch as the 110 photographs which you have produced were not made in the presence of defendant's counsel and representatives I had not intended referring to them or offering them in evidence, but inasmuch as they have been called for by defendant's counsel I will now ask you to state briefly what they show, referring particularly to those marked as follows:—60, 24, 21, 19, 79b, 79c, 56
9011 and 54.

A. In order to obtain the best photographs of the figures in the Braun tube it is necessary that all parts of the apparatus be in good adjustment. There is a chance for many things to happen to cause indistinct photographs. I have picked out of the 110 pictures taken previous to the tests of July 3 and 4 certain photographs, which are not different from the others, but simply show more clearly the results which I am bringing out. Between the time these photographs were taken and the photographs taken during the tests of July 3 and 4 the transmitting set has been entirely readjusted. The results obtained, however, show the same characteristics.
9012 The operation was considered satisfactory while these photographs were taken.

Plates 21 and 19 were taken under approximately the same conditions as were plates BA and BB of the test of July 4 and show the straight line current deflections due to the oscillations in the primary circuit. They clearly show at least two maxima on each side of the zero. Plates 24, 54 and 56 show the combined electromagnetic and electrostatic deflections. Plate 24 shows three half loops on each side of the zero, or, in other words, gives evidence of certainly 2½ and probably 3 complete oscillations. Plates 54 and 56 were taken with the same arrangement of apparatus, 54 being for wave 2 and 56 for wave 1. Both of these photographs show the phenomenon of beats in the oscillations of the current in the primary circuit. There are probably five or six complete oscillations.

For some time the presence of these beats troubled me because the apparatus had not been changed in its adjustment. The reason for the appearance of these beats was due to the fact that the gap was hot.

Plate 60 shows the result obtained when using the combined electromagnetic and electrostatic deflections, 9014
the antenna being disconnected at each end. The figure shows a wave train in the primary circuit of at least six complete oscillations.

Plates 79b and 79c are photographs of a rotating vacuum tube connected with the antenna circuit. The vacuum tube is illuminated every time a discharge occurs in the primary circuit. The vacuum tube was rotating at approximately constant velocity and the even spacing of the images of the vacuum tube give evidence of the regularity of operation of the set. Plate 79b was taken for one discharge of the gap per half cycle of exciting current. Plate 79c shows the rotating vacuum tube when
the gap is discharging twice per half cycle. These va- 9015
cuum tube photographs were taken with the apparatus in the same adjustment for which the previous photographs were taken.

By Mr. Peters: I offer in evidence the 110 photographs last produced and referred to by the witness and ask that they be marked "Chaffee Photographs of Tests prior to July 3".

Q. It appears from your testimony that you have conducted exhaustive tests on defendant's Simpson Mercury

9016 Dr. Emory L. Chaffee—Direct.

Valve transmitter, extending over a period of more than two weeks, and that during such tests you have made approximately 170 negatives showing the operation of this transmitter. I will ask you to state briefly what your conclusion is as the result of these tests, as to whether or not the closed primary spark gap circuit of defendants Simpson Valve transmitter is or is not an oscillatory circuit in its normally operative condition. A. I am perfectly convinced from the tests which I have made that the primary circuit of the Simpson Mercury Valve transmitter is oscillatory. Every photograph which gave
9017 any evidence one way or the other gave evidence of oscillations. There are at least two complete oscillations, and sometimes more.

Q. That is, it is oscillatory in the way you have illustrated in your diagrams, particularly diagrams 2, 3, 5, 6 and 8? A. The photographs have proven that the primary circuit is oscillatory in the manner shown conventionally in diagrams 2, 4, and further represented by diagrams 5, 6 and 8.

Q. In connection with your tests, or one or two of them, you have stated that you short-circuited one side of the mercury rectifier. Do I understand that this had
9018 the effect of taking it entirely out of the circuit? A. I consider that short-circuiting one side of the rectifier has the same effect as taking it out of the circuit. I may add that the keep-alive was not in operation when the mercury valve was short-circuited.

Q. State the circumstances under which the Marconi Company requested you to make the series of tests about which you have testified. A. The Marconi Company asked me to perform these tests to determine the facts concerning the operation of this set. I have not been retained by the Marconi Company and am an uninterested party.

DIRECT EXAMINATION CLOSED.

 9019

CROSS EXAMINATION.

Questions by Mr. Farnsworth.

XQ. Who first communicated with you in behalf of the Marconi Company in this matter and when? A. I am not sure whether it was Mr. Betts or Mr. Waterman, who first communicated with me. It was sometime about the first of June.

XQ. By letter? A. By letter.

XQ. Your tests of the Simpson Mercury Valve transmitter were done "purely from a scientific point of
view"? A. The tests were made purely from a scien- 9020
tific point of view.

XQ. How many Simpson Mercury Valve transmitters have you ever seen? A. I have never seen a Simpson Mercury Valve transmitter prior to this test.

XQ. Where have you seen that transmitter? A. I first saw it in the Cruft Laboratory.

XQ. On what date? A. The apparatus was shipped to me and arrived here, I should say, about the 8th or 9th of June or possibly later. The apparatus remained unopened for several days. I think I first saw it about June 13 or 14. I am not at all sure of those dates.

XQ. What did you know about any Simpson Mercury
Valve transmitter prior to the time you saw this one 9021
unpacked, about the middle of June? A. I had never heard of a Simpson Mercury Valve transmitter previous to my communications with the Marconi Company.

XQ. Which was about? A. Some time about the first of June.

XQ. And what was it you knew about any Simpson Mercury Valve transmitter up to the time when you saw this one for the first time, unpacked about the middle of June? A. The connections and arrangement of the Simpson Mercury Valve transmitter were described to me by one of the representatives of the Marconi Company, Mr. Waterman, before the apparatus reached me.

XQ. How much time did Mr. Waterman spend with you before the transmitter was unpacked? A. Possibly one hour; Mr. Waterman was with me about one hour, or less.

XQ. Here at the Laboratory? A. Here at the Laboratory.

XQ. When was that, about? A. Well, about the first of June, shortly after the first communication.

XQ. Before the transmitter was packed you under-
9023 stood the Marconi Company hoped to show oscillations in the converting trigger circuit of the Simpson Mercury Valve transmitter? A. I understood that they were interested in showing that there were oscillations in the primary circuit of the transmitter.

XQ. Who unpacked the transmitter the middle of June? A. I was not here when the transmitter was unpacked. Mr. Shoemaker of the Marconi Company, together with the Laboratory assistants here I understood unpacked the apparatus.

XQ. Who is Mr. Shoemaker of the Marconi Company? A. Are you asking me that? Mr. Shoemaker is one of the engineers of the Marconi Company.

9024 XQ. Mr. Shoemaker has not been present during these proceedings commencing July 3? A. Mr. Shoemaker was not present July 3 or 4.

XQ. Or 5 or 6? A. Or 5 or 6.

XQ. Besides yourself and your Laboratory assistants what persons have done work on this Simpson Mercury Valve transmitter since it was unpacked here at your Laboratory? A. Mr. Washington is the only one that I know of who has done work on the Simpson Mercury Valve transmitter, other than a student who personally assisted me.

 9025

XQ. Who is Mr. Washington? A. Mr. Washington was an assistant at the Laboratory for some years and is now in business for himself.

XQ. No business relating in any wise to radio telegraphy, I assume? A. Yes; he is in business in radio telegraphy.

XQ. Will you state briefly what the business is?

Objected to as immaterial.

A. Mr. Washington is engaged in manufacturing radio telegraphic apparatus. 9026

XQ. Is it a company, has it a name? A. Under the head of Cutting & Washington.

XQ. I understand then that no one representing the Marconi Company has done any work on this Simpson Mercury Valve transmitter since it was unpacked here about the middle of June? A. Nobody from the Marconi Company has done any work except Mr. Shoemaker, who connected the apparatus when it was first unpacked.

XQ. Please describe in detail just what Mr. Shoemaker did with the apparatus after it was unpacked? A. I wasn't there at the time the apparatus was unpacked, but Mr. Shoemaker, I understand, connected the apparatus. 9027

XQ. By connected, you mean just what? A. Made the connections and put the set in operation.

XQ. And by putting in operation you mean just what, please? A. Adjusted the set for operating.

XQ. And what do you mean, that Mr. Shoemaker knew about putting the Simpson Mercury Valve Transmitter into condition for operating? A. I understood that Mr. Shoemaker knew the proper connections for the Simpson Mercury Valve Transmitter, but I personally traced out all connections.

XQ. After that what did Mr. Shoemaker do, if you recall? A. Mr. Shoemaker was present during a few of the preliminary tests, the first tests.

XQ. Will you please give me, as nearly as you can, the total number of hours which you have worked on this Simpson Mercury Valve Transmitter up to the present time since it was unpacked? A. I have personally put in about 80 or 85 hours on testing the Simpson Mercury Valve Transmitter.

XQ. And can you tell me, please, about how many hours Mr. Shoemaker put in on it after it was unpacked?

9029

Objected to as incompetent.

A. Mr. Shoemaker put in perhaps 10 or 12 hours.

XQ. And what has Mr. Weagant done on this transmitter, the chief engineer of the Marconi Company? A. Mr. Weagant first appeared at the Cruft laboratory July 1st, I believe, and has done nothing but assist in throwing switches, that is, I don't remember any particular thing which Mr. Weagant has done other than assisting me in conducting the tests.

XQ. And Mr. Waterman? A. Mr. Waterman has been here at the tests and has offered some suggestions
9030 and rendered some assistance.

XQ. During what time or times and for how many hours? A. Mr. Waterman was here during some af the first tests for perhaps a day and again July 1st and 2nd.

XQ. And continuously Mr. Waterman has been here since July 1st? A. Yes, he has.

XQ. What did you learn from Messrs. Weagant and Waterman concerning the Simpson Mercury Valve Transmitter? A. Mr. Waterman described to me the connections by the aid of a blueprint, or first by simple

 9031

drawing, and then confirmed by the blueprint, and he explained his idea of the operation of the set.

XQ. His idea being what, as expressed to you? A. Mr. Waterman said that he believed that the primary circuit was oscillatory.

XQ. That was when that he expressed that belief? A. That was during his first interview, somewhere about June 1st.

XQ. Did Messrs. Weagant and Waterman or either of them give you any information about this Simpson Transmitter which was stated to be the result of the experience of them, or either of them, with its operation, 9032
actual operation? A. The only thing which I remember is a statement by Mr. Waterman in confirmation of his idea that the primary circuit is oscillatory, is an experiment which he performed in which a piece of iron was placed in a coil carrying the primary current, and he found that the iron was unmagnetized by the passage of the primary current, indicating the oscillatory nature of the discharge. I believe I have the description of that experiment correct, but I am not sure. I gave no weight to the experiment.

XQ. Do you understand where this Simpson Mercury Valve Transmitter came from before it reached your laboratory? A. I understand it was sent to me by the 9033
Marconi Company from their laboratory.

XQ. You did not understand whether or not Mr. Weagant or Mr. Waterman actually themselves operated this transmitter? A. I did not.

XQ. Do you know of what this particular transmitter consisted, this transmitting apparatus in its entirety as it came from the packing case? What did the apparatus so packed include? A. The apparatus included the main panel upon which was mounted the rectifier and condenser gap tuning giving coils, etc., a motor gener-

9034

ator 500 cycles, two field rheostats, a variable inductance to serve as a dummy antenna inductance, several condenser jars, an extra mercury valve, (which, however, was not used), several protective devices for the generator and other parts of the apparatus, transformer, and a Marconi gap.

XQ. Did you use that Marconi gap in your tests of the Simpson transmitter? A. The Marconi gap was not used previous to July 3rd in the evening. No photographs have been taken with the Marconi gap in place.

XQ. Save for the spare mercury valve and the Mar-
9035 coni gap, the apparatus used in your tests of the Simpson Mercury Valve Transmitter is the same apparatus received in the packing case from the Marconi Company? A. With the exception of a resistance for the antenna circuit for which we used one of the laboratory resistances.

XQ. The generator you used in your tests of the Simpson Transmitter was the generator shipped to you from the Marconi Company? A. It was.

XQ. How about the primary reactance used in your tests? A. I forgot to mention that there was a primary reactance which came with the apparatus which was used.

9036 XQ. Did you understand that the generator which the Marconi Company shipped you with the Simpson set formed a constituent part of the Simpson set? A. I understood that the generator made no particular difference in the operation of the Simpson set.

Question read.

A. No, I didn't so understand.

XQ. Did you observe what that generator was which the Marconi Company sent you with the Simpson set? A. Yes, I did.

 9037

XQ. What was it? A. The generator was a 500 cycle approximately 125 or 150 volt generator.

XQ. Whose was it? Of what radio telegraphic apparatus was it a part, if you know? A. I don't know.

XQ. You didn't observe whether or not it was a generator forming part of the Marconi Company's radio telegraphic outfit? A. I noticed a name plate, the Marconi Company's name plate on the generator.

XQ. Before making any tests of the Simpson Transmitter did you examine the spark gaps therein contained? A. I did not. The spark gaps were not opened before
July 3rd. 9038

XQ. Why were they then opened? A. The operation of the set was so irregular on July 3rd that the tests were unsuccessful. On testing the gaps it was apparent that two were short circuited. I understood that it was normal procedure to open and clean the gaps. The gaps were therefore repaired prior to the tests of July 4th.

XQ. Did the gaps function worse on July 3rd than they had in your previous tests? A. They did.

XQ. When did Mr. Waterman and Mr. Weagant arrive at this laboratory the last occasion? A. They arrived July 1st.

XQ. What did you do or cause to be done to the 9039
sparking surfaces of the Simpson gaps after they were opened on July 3rd? A. They were turned in the lathe and faced off.

XQ. Describe the appearance and condition of the sparking surfaces of the gaps before they were turned off in the lathe. A. Three of the gaps showed a great amount of pitting, there being on one surface a large raised portion from a quarter of an inch to a half inch long, sometimes round, other cases elongated, and on the opposite face a corresponding depression. The rest of the surface of the gap showed evidences of normal sparking.

XQ. You have no relation of any kind, have you, with the Cutting-Washington Co.? A. I have no relation with the company other than having sold a patent to the company.

XQ. That patent was for what? A. That patent was for a system of producing oscillations involving a special spark gap and tuning adjustments.

XQ. That is, the patent was for radio telegraphic transmitter? A. Telegraphic transmitter.

XQ. Has your transmitter of that patent ever been embodied and constructed and operated? A. Yes, it
9041 has.

XQ. That transmitter wasn't capable of being operated, was it, with half an oscillation in the spark circuit?

> Objected to as calling for immaterial testimony; witness is instructed he need not discuss his own apparatus unless he chooses to.

A. It was.

XQ. Then you don't think it impossible for a radio telegraphic transmitter to operate with one half oscillation in the primary circuit? A. It is not impossible.

9042 XQ. On the other hand, it is quite possible? A. It is quite possible.

XQ. If the transmitter is properly constructed and operated? A. Yes.

XQ. You have done that yourself with your transmitter? A. I have done that.

XQ. And proved it? A. And proved it.

XQ. How? A. By the same means.

XQ. Namely? A. Braun tube tests.

XQ. In that case what was the supply, direct or alternating current? A. Direct current.

 9043

XQ. Did you inspect the Simpson spark gaps after the conclusion of your tests on July 4th at any time after that? A. No; I have not inspected them.

XQ. Was any difficulty had in opening or disassembling the Simpson gaps? A. They opened rather difficultly.

XQ. When was that opening or disassembling performed? A. As I have stated, on the evening of July 3rd.

XQ. Who advised that? A. I advised it.

XQ. Did Mr. Waterman or Mr. Weagant have anything to do with operating the Simpson set after they
arrived at your laboratory on July 1st? A. No; except 9044
while assisting me.

XQ. When was that before July 3rd? A. Only as I stated previously.

XQ. Did that happen before July 3rd after they arrived here on the 1st? A. Mr. Waterman was here during the first preliminary tests.

XQ. Was the Simpson set operated after either of those gentlemen arrived on July 1st and before July 3rd? A. It was; yes, sir.

XQ. In their presence? A. Yes.

XQ. As to your transmitter you referred to a moment ago, you have operated that also with an alternating
current supply? A. I don't quite see the bearing that 9045
has.

Mr. Peters: The witness is instructed that he need not discuss the operation of his own apparatus unless he so elects or is instructed by the Court to do so.

XQ. Just as you please; I am on a fishing excursion. A. I do not care to answer.

XQ. What difficulty was had in opening or disassembling the Simpson gaps on the evening of July 3rd? A.

The gaps were opened after having made wrenches for the purpose, and no hammering was necessary in opening the gaps.

XQ. The gaps were not injured in any way in opening them? A. They were not, to my knowledge, injured. I will say that in opening them the raised portion, in being forced over the opposite face, broke the gasket in two cases I believe. In reassembling the gaps the gaskets were reinforced by flowing sealing wax between the two elements of one side of the gap so that the middle sparking surface was held firmly in place and the gap was air-
9047 tight.

XQ. The sealing wax replaced what material that had been present in the gaps as originally constructed? A. The sealing wax did not replace any material, but was flowed in a groove in the back so that the two surfaces were held firmly in their normal position.

XQ. Of what material was the gaskets composed? A. As near as I could determine, the gaskets were made of some natural stone or molded compound. I can't say what.

XQ. You did not attempt to replace the broken gaskets with new gaskets of the same material? A. We did not.

9048 XQ. And you are of the opinion the sealing wax is equally as good as material to remedy the broken condition of the gaskets for the operation of the transmitter? A. I consider the sealing wax entirely adequate unless the gap gets sufficiently hot to melt the sealing wax.

XQ. Didn't the Marconi Company provide you with any wrenches for opening the gaps? A. They did not.

XQ. Has the Cutting-Washington Co. any relation with the Marconi Company? A. Nothing.

XQ. You know that? A. So far as I know.

XQ. No relation of any kind? A. No relation of any kind.

XQ. Why did you advise the opening of the gaps on the evening of July 3rd? A. When I found that two gaps were entirely inoperative and that the set was working irregularly, I was satisfied that the failure of the tests on July 3rd was due to the poor condition of the gap.

XQ. Of course, nobody objected or made any discussion about your proposition of opening the gaps? A. After opening one and having broken the gasket, there was some discussion as to the advisability of opening the rest. Otherwise nothing that I can remember.

XQ. Did you insist upon opening the rest of the gaps 9050
after the first one was opened and the gasket broken? A. After finding that two were short circuited we knew we could do no harm by opening those two gaps. When we found the condition of those and found the reason for the breakage of the gaskets, I concluded that the remaining two gaps could be opened without breakage, and they were.

Question read.

A. Yes.

XQ. Did you consider the rectified charging current used in the Simpson Transmitter to be a direct current? 9051
A. I considered the current to be rectified, but not a constant direct current.

XQ. The Cutting-Washington Company are competitors of the defendant Kilbourne-Clark Manufacturing Company, are they not?

Mr. Betts: Objected to as immaterial.

XQ. They sell commercial apparatus to users? A. Yes.

XQ. With the arrangement of circuits you used with the Simpson mercury valve transmitter, if an arc were in existence across the spark gaps, would that positively eliminate any quenching property of the gaps? A. It is conceivable a portion of the gaps may be arcing, while other portions are quenching.

XQ. Was the pitted and burnt condition of the gaps found when opened any indication that that had been caused by an electric arc across the gap electrodes? A. The pitted portion is evidence of improper working and of very large currents, but I would not say evidence of arcing.

9053 XQ. In connection with your investigation of this Simpson mercury valve transmitter, have you tested any combination of apparatus other than that tested in our presence on July 3 and 4? A. I have not; other than different tuning adjustments.

XQ. The apparatus you tested on July 3 and 4 was the only apparatus which was delivered to you by the Marconi Company? A. It was the only apparatus.

XQ. And on July 3 and 4, it was in exactly the same condition and arrangement as when it was first turned over to you by Mr. Shoemaker? A. Exactly the same.

XQ. In your tests, have you made any attempt to ascertain the potential distribution of the dummy an-
9054 tenna you used? A. No, I have made no tests.

XQ. What voltage did you use during your tests in the primary of the power transformer? A. About 125 to 150.

XQ. What frequency? A. Five hundred cycles.

XQ. What effect did the closing of the key have upon the voltage impressed upon the primary of the power transformer? A. When the key was closed, the voltage dropped somewhat.

XQ. What was the effect of closing the key on the frequency supplied? A. The frequency decreased some, but there was a resistance in the motor field, and the frequency could be varied.

XQ. How do you know the voltage dropped when the key was closed? A. By the reading of the volt meter.

XQ. You read it? A. Yes.

XQ. Where did that extra mercury valve come from, do you know? A. I don't know.

XQ. It came from the Marconi Company with the apparatus, packed up? A. Yes.

XQ. Were you informed whether the mercury valve you used in your tests was the one which the defendant company furnished the plaintiff company with the trans- 9056
mitter itself? A. I was not informed one way or the other. I assumed it was.

XQ. Where is the second mercury valve? A. In the room with the apparatus.

XQ. What was the decrement of the dummy, or artificial antenna which you employed in your tests? A. The resistance was approximately six ohms. The inductance was such as to make the natural period of the dummy antenna about 230 metres. One can easily calculate the decrement of the antenna from those constants.

XQ. Have you done so? A. I have not.

XQ. How did you know what the resistance of the
dummy antenna were? A. The resistance consists of four 9057
ohms, of a laboratory standard, plus two ohms, assumed for the two jars.

XQ. What was the capacity of the dummy antenna you used in your tests? A. The capacity was .001 microfarads.

XQ. What was the resistance of the condenser in the dummy antenna you used in your tests with the Simpson transmitter? A. I assumed it two ohms.

XQ. Those were Marconi Leyden jars you used with the dummy antenna? A. They were.

9058 Dr. Emory L. Chaffee—Cross.

XQ. What do you know about them? A. Nothing more than what I have said.

XQ. They brushed quite a bit in your tests, didn't they? A. They did.

XQ. What was the resistance of the condenser in your dummy antenna? A. Two ohms; I have assumed it to be two ohms.

XQ. Who was it; was it Mr. Shoemaker, you said, who put the transmitter into operating adjustment for you? A. I did, but I completely readjusted it after he left.

9059 XQ. So you didn't depend on Mr. Shoemaker? A. Not in the least.

XQ. You didn't depend on anybody except yourself? A. I didn't depend on anybody but myself.

XQ. During your tests, you considered the Simpson set in good operating adjustment? A. I did.

XQ. Just what do you mean by that? A. I mean the Simpson set was adjusted for maximum radiation, consistent with best tone. It was found that maximum radiation and best tone occurred with the same adjustment.

XQ. On July 3 and July 4, you personally observed, in connection with each photographic test the fact of the
9060 maximum radiation and good tone? A. The adjustments of the set were made previous to the test so that maximum radiation and best tone were obtained. The tuning adjustments were not changed during the test, and I assumed them to be in good adjustment, although I personally observed the radiation and the tone several times during the test.

XQ. You believed that on July 4 the adjustments were such as to give a good tone to the note? A. I have observed throughout the tests that the tone may be good one instant, and be not so good the next instant, with no change in the adjustment. I have also observed during the long series of tests that the results obtained for the primary circuit are unaffected by any slight change in radiation or quality of tone.

XQ. Did you make any tests with the Simpson transmitter, using instead of your dummy antenna in the laboratory one of the radial antennae on the roof of this building? A. I did not.

XQ. There are two such on the roof of this building? A. There are, but because of the length of the tests I did not feel justified in disturbing the neighborhood.

XQ. It is your belief, isn't it, that the signal notes 9062
obtained during your test with this transmitter were those which occur in the commercial use of the Simpson transmitter? A. I don't understand what you mean by "signal note".

XQ. The note we have been talking about. A. The note? I was informed that 500 cycles were used with the Simpson transmitter.

Mr. Farnsworth: Please repeat the question.
(Question read.)

A. Do you mean quality, or pitch?

XQ. Both, or either. A. Yes. 9063

XQ. What wave meter did you use to determine whether the tone was good, in your test? A. I used a Pierce wave meter, and a wave meter made by the General Radio Company.

XQ. Has Prof. Pierce had anything to do with this Simpson mercury valve transmitter, or the tests thereof? A. Nothing except to observe the results of one or two of the tests, confirming my conclusions.

XQ. You didn't get any information from Prof. Pierce as to the normal commercial conditions of use of the Simpson transmitter? A. I did not.

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XQ. From whom did you obtain any such information? A. I haven't obtained information from anybody.

XQ. In other words, when you speak of the normal operating conditions of the Simpson mercury valve transmitter, you mean what you assume the same to be? A. That, together with the fact that I was told that 500 cycles was ordinarily used, by the representative of the Marconi Company.

XQ. Namely? A. Mr. Waterman.

XQ. You understand that Mr. Waterman is entirely familiar with the conditions of commercial use of the
9065 Simpson mercury valve transmitter? A. I assume so.

XQ. And Mr. Weagant? A. I assumed he was, also.

XQ. You, I take it, consider that the transmitter of your own design is better than Mr. Simpson's transmitter, in respect of the limited or short duration of action in the primary? A. I do.

XQ. Has your transmitter had any extensive commercial use?

Mr. Betts: Objected to, as immaterial.

Mr. Peters: You need not answer, unless instructed by the Court.

9066 XQ. I take it you have completely disposed of your interest in your transmitter?

Mr. Peters: Same objection and instruction.

XQ. From the fact of what you said about having sold the patent to the Cutting-Washington Company. A. I decline to answer.

XQ. May I ask what your transmitter is called?

Mr. Peters: Same objection.

XQ. Is that the Chaffee gap transmitter? A. So-called.

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XQ. Who selected, or took part in the selection of the first batch of photographs you produced in evidence, consisting, I think, of about thirty odd? A. I have selected personally all of the photographs submitted in this case.

XQ. Without any assistance, or suggestions, or advice from any one? A. Without anything other than approval.

XQ. Those 110 photographs taken prior to July 3 which you produced; do those include the negatives and the box which we had down stairs marked "N G"? A. Yes.

XQ. Including all you took? A. All, yes. 9068

XQ. So far as you know, there was no change of any sort made in the Simpson apparatus or any part of it from the time you first adjusted it after Mr. Shoemaker had turned it over to you, up to the evening of July 3? A. There was no change to my knowledge.

XQ. What were all the changes made after your tests on the day of July 3 were concluded and before the tests of July 4 were commenced? Specify all that were made. A. The spark gap, as was stated, was repaired, and the tuning adjustments were slightly altered, to obtain the best radiation and best tone. The set had hitherto been operating on less than the normal number of gaps. 9069

XQ. And what were all those slight adjustments you referred to made between the tests of July 3 and July 4? A. I can't say whether the adjustments were appreciably altered or not. Various adjustments were tried, coming back always to the one which gave most satisfactory operation, and I am convinced that they are essentially the same as were the adjustments in the previous tests.

XQ. I take it from what you have said that none of the conditions of the Simpson apparatus existing during your tests of July 4 was the result in any wise of any

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suggestions or advice from Messrs. Weagant or Waterman? A. No, they were not.

XQ. All your own? A. All my own.

XQ. And not anybody else's at all? A. Nobody else.

XQ. Excepting the Simpson mercury valve transmitter, you have seen under conditions of commercial operation other radio telegraph transmitters? A. I have.

XQ. Did you ever see one using a mercury valve? A. No.

XQ. You stated in your direct-examination that on account of the nature of your tests and the limitations of
9071 apparatus, it was difficult to tell just how many oscillations there may be in the converting trigger circuit of the Simpson mercury valve transmitter. What do you mean by that, "the nature of the tests and the limitations of apparatus"? A. The intensity of the central spot, as compared with the intensity of the deflected spot was so great that it is impossible to see the dim deflections which might occur near the central image. The presence of the deflections distant from the central image, is unquestionable, and gives conclusive evidence of the existence of oscillations. As the oscillations die down, the smaller oscillations are undistinguishable from the central image.

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XQ. There is no blurring on the photographic negatives taken in your July tests, is there? A. Yes, some of them are blurred.

XQ. Well, is it true in all cases that blurring is due to the irregularity of the action of the transmitter, as you testified on direct? A. No, the blurring may be due to the condition of the Braun tube.

XQ. So that the blurring may be due to irregularities either of the action of the Braun tube or of the transmitter itself? A. Yes.

XQ. Will you please describe in detail the character of Mr. Weagant's assistance to you during the tests of July 3 and 4? That is, you say he operated the switches. What were all the things he did, as a result of operating switches and what not? A. I cannot recall all that Mr. Weagant did, but I am very sure that he made no suggestions or did anything which altered my natural course of conduct in the tests.

XQ. Would you say that the various operations which you conducted, the tests of July 3 and 4 were those normally done with a transmitter by an operator on shipboard? A. The tests in which plates B A, B B, B C, B D and B E and B F were taken were made with the apparatus in normal adjustment immediately after the gaps had been repaired, and as the set would be used normally on shipboard. Toward the end of the series of tests, the set was operated with the antenna off, which would not normally be done.
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XQ. Have you enumerated all those things which Mr. Weagant did during your tests of July 3 and 4? A. I have.

XQ. What were they? A. Mr. Weagant, under my direction, closed the field switch of the generator, and made certain adjustments of primary reactance, dummy antenna inductance, and short-circuiting of gaps, always under my direction.
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XQ. What did those acts of his result in, respectively? A. I don't know what you mean.

XQ. You don't know what I mean? That is to say, he did those things, you say, and what was the result of those acts of his on the operation of the apparatus; transmitter? A. I can't answer that question in full, or completely. The adjustments were made to bring out several points which I wished to illustrate in the tests.

XQ. Well, he changed the frequency, didn't he? A. No.

XQ. Didn't change the frequency at all? A. No.

XQ. Didn't change the number of sparks per second? A. Yes.

XQ. Didn't change the number of discharges per half cycle, or per cycle? A. Yes.

XQ. Didn't change the tone of the note? A. Yes.

XQ. Well, now, Dr. Chaffee, that is what I was inquiring about. You say he did certain things. Now, you say that certain things resulted. Now, won't you please— A. I didn't understand you.

XQ. I beg your pardon; I thought it was perfectly
9077 clear. Will you do that now? A. The several adjustments which I asked Mr. Weagant to make altered the number of discharges per half cycle of primary charging current; altered the regularity, and therefore the tone of the transmitting set; altered the potential to which the condenser "C" was charged; but in every case the adjustments were so made as to obtain the best radiation and tone from the transmitting set.

XQ. And did Mr. Weagant use the field switch as a key? A. Yes, he did.

XQ. And he adjusted the tone by varying the field rheostat? A. That and the reactance in the primary.

9078 XQ. You understand it to be the normal duty of a radio telegraph operator on board ship to change his spark frequency; that is, the number of discharges per half cycle, or cycle? A. I understand that the Simpson mercury valve transmitter is designed to operate for either one discharge per half cycle, or two discharges per half cycle.

XQ. That being within the control of the telegraph operator? A. I don't know, but in any case, the observations which I have made are independent, so far as the oscillatory character of the discharge in the primary is concerned, whether the condenser "C" discharges once

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or twice or a greater number of times per half cycle of charging current.

XQ. Did you have any apparatus which you used in your tests of July 3 or 4 which was auxiliary, or additional to those used by a radio telegraph operator; that is. of course, excepting the Braun tube? A. The usual radiating antenna was replaced by a dummy antenna. I don't know whether it is customary with the Simpson transmitter to use a reactant in the primary circuit.

XQ. How about the stroboscope you used? A. The stroboscope was additional, and used to determine, in part, the regularity of the operation of the set. That, 9080
together with the wave meter, was used to determine the quality of the tone.

XQ. In your tests on July 3 and 4, you personally observed either the wave meter or the stroboscope, in order to determine the tone, at the instant you took the photographs? A. On July 3, I don't remember of listening to the tone. On July 4, I personally observed the tone and the stroboscope, and the photographs were taken immediately after.

XQ. The times you pressed the camera bulb, were you watching the stroboscope, or listening in to the note, one or the other, at the time you
took each photograph; at the instant you pressed the bulb 9081
for each photograph? A. I was not, but from the audible sound which one can hear in the room, there was no appreciable change.

XQ. Then you relied upon that audible tone of what? A. The sound which is emitted from the condenser, and other sounds.

XQ. What other sounds? A. A sound is emitted by the spark gap when the set is not operating properly, which can be recognized by one familiar with the operation of a set. Judging from that, and the sound of the condensers, one can judge fairly of the regularity of operation of the set. I wish to point out, with considerable emphasis, that the observations in the Braun tube, although somewhat blurred if the set is not operating regularly, are of exactly the same character as when the set is emitting a clear note.

XQ. What condenser was it that you mean you heard? A. The condenser in the dummy antenna circuit.

XQ. Well, at certain instants you pressed the camera bulb. In your own mind, what determined that particular instant? Was it determined by the sound you heard in
9083 in the room? A. No. The camera bulb was pressed when the figure in the Braun tube showed least blurring.

XQ. You were watching the screen of the Braun tube, with your hand on the camera bulb? A. Yes; I was going primarily by the figure on the Braun tube.

XQ. And for a given test, you gave Mr. Weagant instructions as to the condition you wanted him to produce? A. Yes.

XQ. Telling him, for example, what? A. The different tests were varied according to whether three gaps or five gaps were used, or according to the adjustment of the reactance in the primary circuit, to obtain the best
9084 tone.

XQ. And according to what frequency of spark discharge you wanted? A. According to the frequency of the spark discharge I desired.

XQ. Anything else? A. Not that I recall at the moment.

XQ. And as you pressed the camera bulb, you were relying upon Mr. Weagant's having effectuated your instructions just previously given to him? A. No.

XQ. And the observation of the screen, too? A. Yes.

XQ. Those two? A. Yes.

XQ. Among the other things you heard, which did not

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control the instant of your pressing the camera bulb, did you hear the spark gap of the Simpson transmitter? A. The spark gap can be heard in some of the tests, particularly when the antenna is not connected on.

XQ. Was it heard by you during the times and did that control the instant of pressing the camera bulb? A. That was heard during the test, particularly in the test in which the antenna was disconnected. I am not sure whether the sound of the spark gap can be heard while the antenna is connected. I am not sure if it can.

XQ. During the tests, what other noises were there present in the room when you heard the spark gap, and 9086
how far away were you from the spark gap when you were pressing the camera bulb?? A. I was about six feet from the spark gap.

XQ. Will you please mark on your diagram No. 1 the distance of the leads from the Simpson mercury valve transmitter to the Braun tube apparatus, and intermediate apparatus, and testify while you are doing so, stating what you are doing? A. The Braun tube was approximately six feet from the transmitter, and I have marked the four leads, leading from the transmitter to the Braun tube as being six feet long.

They may have been nearer eight. Shall I re-mark the diagram? I will re-mark the distance, as being nearer 9087
eight feet.

XQ. Have you any clear recollection of the lengths of time you observed the images on the Braun tube screen for a given observation on the tests of July 3 and 4, relative to the time you pressed the camera bulb, or were you so interested that you have not a clear recollection of those relative times? A. I have no clear recollection, but I should say I observed the image on the screen perhaps sometimes two or three times longer than the camera bulb was pressed, and sometimes very little longer than the camera bulb was pressed.

XQ. On many occasions you recall that the image on the Braun tube screen, under a given set of adjustment by Mr. Weagant varied as you observed it? A. The figure on the screen did not vary in character, but simply in sharpness or definition, and the adjustments which Mr. Weagant made were entirely under my direction, and confirmed by me.

XQ. Yes, but I mean, speaking of a given set of adjustments by Mr. Weagant given observations by you of the conditions on the screen, what was happening to the image there when you were watching it, while you were
9089 observing it and before you finally pressed the camera bulb? A. The figure on the screen in some cases became clearer after the set had been in operation for a short time, so that the generator had attained constant speed, and had settled down to the condition under which it was primarily, or normally, adjusted. I waited before taking the exposure until the transmitting set had settled down into normal, steady operation, which I judged primarily from the appearance of the figure in the screen, but very often, as I pointed out, from the noise or the sound which I heard from the set.

XQ. Do you recall how many occasions during the test of July 3 and 4 there were when you made visual obser-
9090 vation of the screen images, without making any photographic record thereof? I mean, successive occasions of observation, corresponding to respective settings or adjustments by Mr. Weagant? Do you remember there were a number? I am asking you how many such occasions there were, when you made no photographic record of your observations? A. There were many cases when no photograph was taken on July 3, because of the irregularity of the set. On July 4, as I remember, there were relatively few occasions when a photograph was not taken, and observations made.

 9091

XQ. About how many occasions of omission to photograph? A. During the first part of the test in which the adjustments were more easily made, there were, I should say, not more than three or four out of seven or eight occasions when a photograph was not taken. I am not absolutely sure on that point. During the latter part of the test—

XQ. On July 4? A. On July 4, the adjustments were more difficult, and there may have been about an equal number of trials when no photographs were taken, and occasions when photographs were taken.

XQ. An even number; how many of each, about? A. 9092
Fifteen.

XQ. Fifteen of each? A. Fifteen of each.

XQ. Now, out of those approximate 17 or 18 when no observations were made— A. Possibly; that is very indefinite.

XQ. Out of that approximate number of occasions, somewhere between 15 and 20 according to your testimony, on July 4, when no photographic record was made of the observations, will you please enumerate the conditions existing; that is, the conditions of the radio apparatus and the resultant images on the Braun tube screen? A. The images on the Braun tube screen on those occasions on which we took no photographs were of the same 9093
character as those photographed, but less distinct. It is customary in scientific work to continually readjust a difficultly adjusted piece of apparatus, in order to get the best results, so I consider that I did nothing different from ordinary scientific practice.

XQ. In your direct examination, you referred to "The results which I wished to bring out." In those tests, what result did you wish to bring out? A. I wished to prove the oscillatory character of the discharge in the primary circuit which I had found to be always the case in my preliminary tests.

XQ. Of the Simpson mercury valve transmitter? A. Of the Simpson mercury valve transmitter.

XQ. Do you recall that the weather on July 4th was more humid than that on July 3? A. I believe July 4 was more humid, but I had taken particular precautions to re-insulate the high tension line coming from the other laboratory.

XQ. About how many photographs have you taken in all of the tests of the Simpson mercury valve transmitter? A. I have taken about 170.

XQ. Do you consider that you are familiar with prac-
9095 tical, everyday operations of radio telegraph apparatus? A. I consider that I am reasonably familiar with everyday operations of telegraphic operators.

XQ. You consider, don't you, that the tone emitted by the Simpson transmitter on July 4 was a good, clear tone, or note? A. I do not consider it a good clear note. I have endeavored, throughout my experiments, to obtain a better note, and I am quite satisfied that with the arrangement of apparatus, I could not better the quality of the note emitted. At times, even without changing the adjustments, the note would be everything to be desired, but the next instant it would change. I have concluded
9096 that the gap, working under such high current density as is the gap of the Simpson mercury transmitter, to be unable to work continuously and regularly.

XQ. I have not the slightest doubt, Dr. Chaffee, that you in good faith did your best to get the best possible results with this particular Simpson mercury valve transmitter in its present condition. You believe you did so? A. I believe I did so. I further believe that were it possible to get continuously a very clear note, that the results which I would then get on the Braun tube screen would be identical, except clearer, than the results which I did get, because I have made observations on the tube at such instances, when the note was particularly clear. I therefore have laid no particular stress, so far as the results of my work are concerned, on the extreme pureness of the tone obtained.

XQ. Can you produce a photographic record of such observations on occasions when the note of the transmitter was perfectly clear? A. For those observations I cannot produce any photographs, in which I am positive that the note was clear. As I have just stated, I am positive that the appearance on the screen is identical with the results which I have photographed. The duration of time for which the note is clear is usually short, 9098
and it would be more or less coincidence if I were able to obtain a photograph at the time when the note was at its greatest purity.

XQ. Have you any idea how many oscillations there are in a single train in a ship's antenna, connected to a normally operating Simpson mercury valve transmitter? A. I don't know; but I should judge about 100.

XQ. You haven't made any such observation? A. I haven't made any observations.

XQ. You were not called upon by the Marconi Company to observe or record the relative number of oscillations in the two circuits of the Simpson transmitter, 9099
namely, the converting trigger circuit, and the antenna circuit? A. No, I have not.

XQ. I mean, of course, under conditions such as used in practice. A. No.

XQ. When you were making your tests, have you any idea how many oscillations you got in your dummy antenna? A. I have made no observations of the number. There were at least 50, probably.

XQ. But not as many as you think would take place under commercial conditions in the ordinary ship's antenna? A. Yes, I consider the two cases entirely equivalent. If I have overestimated the number in the ship's antenna, I do not wish that to be construed to mean that there are less than the normal number in this case.

XQ. In your tests with the Simpson transmitter, that is, this particular one here, in its present condition, you said in connection with "Chaffee diagram 6" that "It is sometimes possible to see five half loops?" A. Yes.

XQ. Under what conditions? A. Under what I consider to be normal conditions.

XQ. In your tests of this particular transmitter in the condition in which it reached you have you observed as
9101 many as 10 oscillations in the converting trigger circuit? A. I have never observed as many as 10 oscillations in the primary circuit except when the secondary or antenna circuit is disconnected. I have observed more oscillations than two complete oscillations in the primary circuit under two conditions; first when the gap is hot, second, when the set is slightly thrown out of tune.

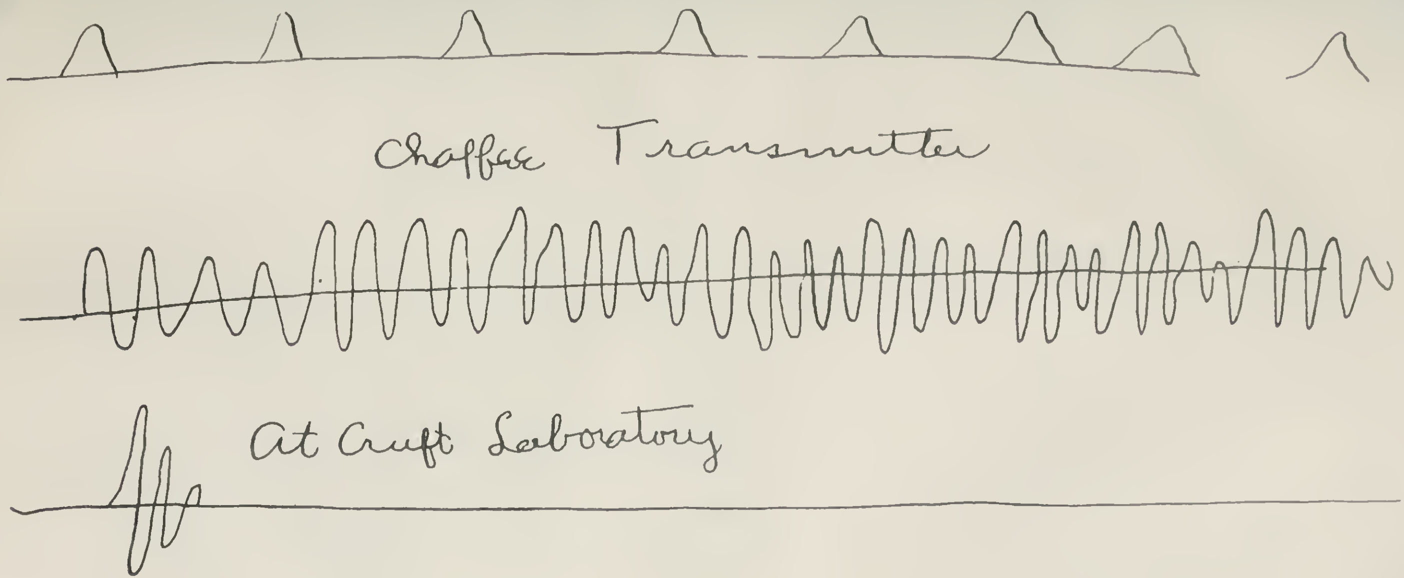
XQ. Have you produced any photographic records by which you intended to illustrate the number of oscillations occurring in practice in a ship's antenna in connection with the Simpson Mercury Valve transmitter? A. I have not, only in so far as I consider the dummy antenna equivalent to the ship's antenna. I have pro-
9102 duced photographs of the oscillations in the dummy antenna, but because of their closeness it is impossible to count them.

XQ. You haven't, have you, produced any photographs which would indicate to any unskilled person the fact that with the Simpson transmitter there may be 50 to 100 oscillations in the antenna? A. I have not.

XQ. What are the titles of the photographs which you have produced which relate in any wise to the conditions in the dummy antenna of your tests? A. BI, BJ, BK, BL and BM.

3035

Aug. '51


Chaffee Transmitter
At Cruft Laboratory
Mercury Valve Transmitte

 9109

XQ. And you say none of those photographs shows 50, much less 100 antenna oscillations, does it? A. It is impossible to tell from those photographs how many oscillations there are.

XQ. Referring to this Simpson transmitter which the Marconi Company has furnished you, you say you don't know where it came from? A. No.

XQ. You don't know its previous history? A. I do not.

XQ. Don't know who has used it? A. I do not.

XQ. You don't know whether the Marconi Company used it before you got it? A. I do not. 9110

XQ. Or abused it? A. No.

XQ. You say in the condition in which you have had it you have observed as few as how many supposed oscillations in the converting trigger circuit? A. I have observed as few as two complete oscillations.

XQ. And although you don't know you say you believe that in an ordinary ship's antenna with the Simpson Mercury Valve transmitter there would be how many oscillations in an antenna? A. Probably 50, possibly more.

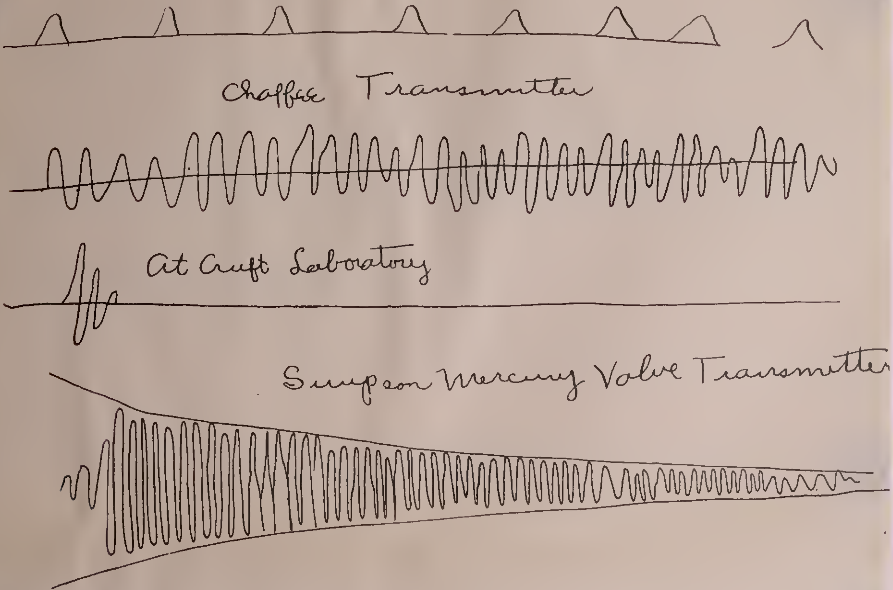
XQ. What would you select as a fair number from your knowledge as a man, as you say, familiar with present day radio telegraphic conditions? A. I should 9111 say 50, perhaps.

XQ. Not more than 50? A. I don't want to restrict myself.

XQ. All you want to do is to be fair? A. I should say 50, about.

XQ. Please make a sketch showing those conditions you have just testified about as to the Simpson Mercury Valve transmitter, the sketch to show those minimum two complete oscillations you observed in the converting trigger circuit and the number of oscillations which you have taken as a fair number. A. I have done so (reproduced opposite).

Def Ex. 51



D Chaffee 11

9112 XQ. Label that please "Simpson Mercury Valve transmitter at Cruft Laboratory." Now, on the upper part of that sheet draw similar sketches showing the oscillations in the two circuits as you have heretofore testified to in Chaffee Gap transmitter. A. There are many sketches I might draw. (Witness makes sketch.)

XQ. And will you label that at the top "Chaffee Gap Transmitter?" A. I have done so.

XQ. Have you counted the antenna oscillations at the bottom of the Simpson sketch? A. No.

XQ. About how many are there there? A. I don't know.

XQ. Just count them. A. There are about 30.

9113 XQ. I hate to trouble you, Dr. Chaffee, but first off I will mark that sketch in evidence "Defendant's Exhibit Chaffee Diagram 10." (Deft's. Ex. No. 50.) You say you have shown 30 there at the bottom of the Simpson sketch. Just mark that 30 antennae. A. I don't wish any importance to be attached to the exact number of oscillations which I show or say exist in a ship's antenna. The number of oscillations depends upon the constants which are quite different in different cases. Do you wish me to mark this 30?

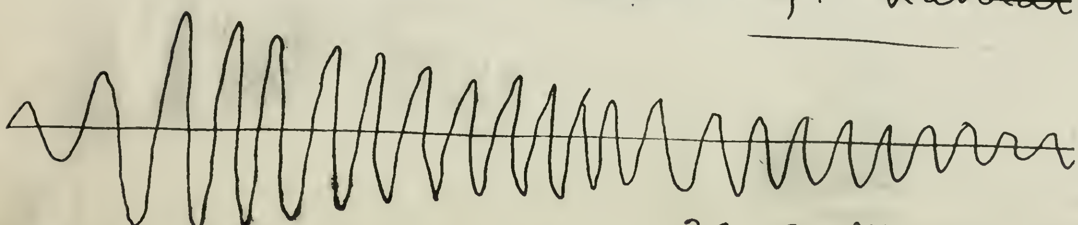
XQ. Thirty, please. A. (Witness marks sketch.) That has no significance whatever.

9114 XQ. I am sorry to trouble you, but inasmuch as you suggested 50 antenna oscillations I will ask you to please draw, Dr. Chaffee, diagram 11 which shall be in all respects, if you please, identical with Chaffee Diagram 10, only showing 50 antenna oscillations, please. A. I have done so (reproduced opposite).

> By Mr. Farnsworth: I offer this in evidence as "Defendant's Exhibit, Chaffee Diagram 11." (Deft's. Ex. No. 51.)

XQ. Bearing in mind that I asked you to make your diagram 11 the same in all respects as diagram 10 save

Simpson Mercury Valve
~~at Croft Laborat~~

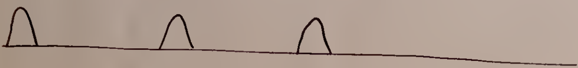


30 antenna oscillations.

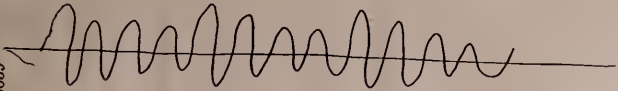
Challes. 10.

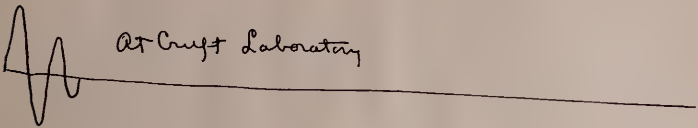
3039

Case No. 718
Defendants Exhibit 50
United States District Court
Western Dist. of Washington.
Marconi
vs
Kilbourne & C. Mfg. Co
July 18 16.
T. J. Crosby



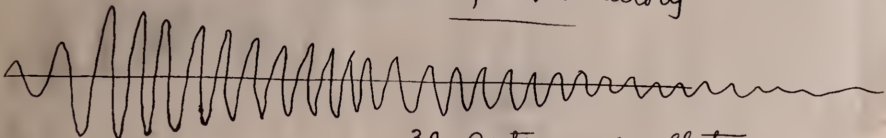
Chaffee Gap Transmitter.





at Cruft Laboratory

Simpson Mercury Valve Transmitter
~~at Cruft Laboratory~~



30 antenna oscillations.

Chaffee 10.

 9121

only as to the number of antenna oscillations; can you say how many oscillations you showed in your diagram 11 as to the converting trigger circuit of the Simpson Mercury Valve transmitter at the Cruft Laboratory? A. I don't remember whether I showed two or three.

XQ. Two or what? A. Two or three.

XQ. Do you remember that I asked you to show two in the diagram No. 10? A. You did. I didn't remember that.

XQ. Looking at the diagram 11, how many have you shown in the converting trigger circuit? A. I have shown two and a half. 9122

XQ. Will you in a still further respect make Diagram 11 like Diagram 10, in respect to the lettering as to the Simpson Mercury Valve transmitter, which is marked on Diagram 10 as being the one at the Cruft Laboratory —will you mark that also on Diagram 11. A. I have never measured the number of oscillations in the antenna circuit in the Simpson Mercury Valve transmitter at the Cruft Laboratory. I have further stipulated with emphasis that there is no significance to be attached to the number of oscillations which I have shown or which I have stated.

XQ. What difference is there between marking "At Cruft Laboratory" on your Chaffee Diagram 10 and marking "At Cruft Laboratory" on your Diagram 11? 9123 A. There is no difference.

Q. Then are you unwilling, having marked "At Cruft Laboratory" on No. 10 to now mark it on No. 11? A. I am not unwilling provided it is not assumed by these drawings that I assert that this is the nature of the oscillations in the antenna circuit or the dummy antenna circuit of the transmitter at the Cruft Laboratory. These were merely guesses at what might happen. Under those circumstances I will mark this "At the Cruft Laboratory."

9124

XQ. Just a moment, please. Your point is that it is not fair to mark "At Cruft Laboratory" on either diagram as applying to the antenna oscillations? A. It is not fair to assume that I am stipulating that there is a number of oscillations in the antenna circuit at the Cruft Laboratory, or that I know how many oscillations there are without calculation.

XQ. That being the fact I would suggest that you cross off "At the Cruft Laboratory" from Chaffee Diagram 10 and indicate on both diagrams 10 and 11, that "At the Cruft Laboratory" should apply to the oscilla-
9125 tions shown as existing, as you believe, in the converting trigger circuit. A. (Witness marks sketch.)

XQ. You got the impression, did you, Mr. Chaffee, that the Simpson Mercury Valve transmitter was to be used by telegraph operators as a given transmitter on either one of at least two spark frequences? A. I understand that the Simpson Mercury Valve transmitter was designed for a spark frequency of one spark per half cycle and that at least in the sets operated with a lower frequency supply source they were operated at several discharges per half cycle.

XQ. What information did you have as to any Simpson Mercury Valve transmitters operating at a lower
9126 spark frequency? A. In the two cases which I have described the spark frequency might be the same or approximately the same.

XQ. I mean generator frequency? A. Generator frequency?

XQ. Yes. A. The representative, Mr. Waterman, told me that the Simpson Mercury Valve transmitters have been operated on lower generator frequency.

XQ. But you have never seen or been provided with such an outfit of Simpson transmitter? A. I never have; no.

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XQ. Have you any idea why you have not been so provided? A. I can see no reason why the oscillatory nature of the primary circuit can depend upon the frequency of the generator.

XQ. Have you any idea, has the question been canvassed at all as to the omission to provide you with a lower frequency generator with the Simpson transmitter; that is, lower than 500 cycles? A. I don't understand what you mean by "canvassed."

XQ. Discussed, between you and any of the Marconi representatives. A. The Marconi representatives told me that it is now customary practice to operate the Simp- 9128
son Mercury Valve transmitter at 500 cycles and that other tests in the case had been so conducted.

XQ. Yes, but, Dr. Chaffee, the inquiry goes to the matter of generators of lower than 500 cycles. A. Did I not answer the question?

XQ. And the question is, has the matter been canvassed or discussed or talked about between you and the Marconi representatives in respect to the omission to provide you with a Simpson transmitter which should be operated by and with a generator of lower frequency than 500 cycles? A. The matter has not been discussed except the statement on their part that the sets have been operated, at lower frequency, but that it would be satisfactory 9129
to conduct the tests at 500 cycles.

XQ. You personally have no information whatsoever as to whether or not the Marconi representatives themselves were or are able to furnish you with a defendant's Simpson Mercury Valve transmitter set including a generator of a frequency lower than 500 cycles? A. I don't know.

XQ. You don't know anything about it? A. No.

XQ. Nothing was said by the Marconi representatives about that? A. No.

XQ. When you said on direct examination that the current activity in the converting trigger circuit of the Simpson Mercury Valve transmitter was only 1/1000 of the time, what did you mean? A. The wave length of the antenna circuit is approximately 600 meters. One complete oscillation requires 1/500000 of a second. Two complete oscillations would require 1/250000 of a second. The spark frequency, if the set is operating to discharge once per half cycle is 1000. Consequently, there is 1/1000 of a second between primary discharges and each primary discharge lasts 1/250000 of a second. That makes the
9131 duration of the primary 1/250 of the time interval between primary discharges; so that my mental estimate of 1/1000 was not quite correct in that connection, 1/250 being perhaps more nearly correct. I had forgotten for the instant whether the 1/1000 was the relative time of duration of one primary train or one half loop relative to the time interval between primary discharges. One half loop takes place in 1/1000 of the time interval.

XQ. What do you mean by the time interval? A. Time interval between primary discharges. That 1/1000 of a second is the time taken by the spark in making a portion of the photographs; in other words, in describing
9132 one of the half loops which we have been examining in the photographs.

XQ. So that in the case of this particular Simpson transmitter now in your laboratory and in the condition in which you received it and the number of trigger circuit oscillations which you have observed, that activity in the trigger circuit is, you say, now, 1/250 of the time between successive discharges in that trigger circuit? A. Provided the spark frequency is once per half cycle.

XQ. Yes, and under those conditions, and in the case where the antenna oscillations number one hundred say, what proportion is that occurrence of 100 oscillations to the total time interval between successive discharges in the trigger circuit? A. If the antenna oscillates 100 times before dying down to less than one per cent., we will say, of its initial amplitude, the time required for the complete antenna train I make 1/5000 of a second. This is one-fifth of the time interval between primary condenser discharges.

XQ. Did your colleague Professor Pierce of Harvard University have a part ownership interest in your Chaffee Gap Radio Telegraph transmitter?

Same objection. 9134

A. Yes.

XQ. And you have understood that he sold out his interest to the Cutting & Washington Company?

Same objection.

A. I decline to answer those questions.

XQ. Have either you or Professor Pierce any stock interest in the Cutting & Washington Company?

Same objection.

A. I decline to answer.

XQ. What were the nature of the tests and the limi- 9135
tations of the apparatus which made it difficult, as you have testified, to determine just how many oscillations there actually may occur in the trigger circuit of the particular Simpson Mercury Valve Transmitter which you have in your laboratory here? A. The difficulty in observing the oscillations is partly due to the inherent irregularity of all similar spark transmitters. This is not only true of the Simpson transmitter but of the other transmitters which I have tested. The other point which makes the photographs and observations difficult is the point

which I have already brought out, that the activity in the primary is of short duration that the deflections are very dim and difficult to see unless large currents are used to excite the Braun tube.

XQ. Referring to the particular sample of Simpson Mercury Valve Transmitter here in your laboratory and in its general present condition, do you think it is impossible under any conditions of adjustment of it to obtain as small a number of oscillations in the converting trigger circuit as you can get in your Chaffee gap transmitter primary? A. I consider it entirely impossible to so ad-
9137 just the apparatus that only one-half loop of current discharge will appear in the primary circuit.

XQ. As in your Chaffee gap? A. As in the Chaffee gap system.

XQ. You are speaking of the particular Simpson Transmitter you have in your laboratory, never having seen any other? A. I am.

XQ. In order to obtain only one half oscillation as in the Chaffee Transmitter primary, do you think you would have to have different conditions than existed in the particular sample of the Simpson Transmitter you have here in your laboratory? A. I think you would.

XQ. And of course I assume that you believe that the
9138 sample of Simpson Transmitter which has been delivered to you by the Marconi Company is, in fact, a fair sample of the defendant's Simpson Mercury Valve Transmitter as used in practice? A. I have assumed so. I was asked to find out the facts concerning the particular transmitter with which I was supplied.

XQ. And you don't know and are not concerned with the question of whether or not it has yet or ever will be proved in this case that the particular transmitter you have inspected here is or is not a fair representation of the Simpson Mercury Valve Transmitter as accepted in commerce? A. That doesn't concern me.

XQ. All you are testifying about and reporting on here is in respect only to this particular transmitter here? A. Yes.

XQ. Have you pretended in anywise in this deposition to represent to the Court that the report and testimony you have made concerning this particular transmitter here necessarily applies to other Simpson Mercury Valve Transmitters such as are sold by the defendant company? A. Yes, I believe I have.

XQ. You have? A. Yes.

XQ. You include in your testimony reference to all Simpson Mercury Valve Transmitters? A. If this is a 9140
fair representative.

XQ. But I thought you said you don't know whether or not it is a fair sample of the transmitters of the Simpson type which are sold by the defendant. A. I don't only in so far as I have been told so.

XQ. By whom? A. By the representatives of the Marconi Company.

XQ. Namely? A. Mr. Waterman.

XQ. Do you wish Mr. Waterman to go out to Seattle and face the Federal Judge in the State of Washington and represent to him that you have made a sweeping assertion that all Simpson Mercury Valve Transmitters
of the defendant company possess the characteristics and 9141
give the results of the particular one you have examined here which was furnished you by the Marconi Company? A. I have not said so, and I don't.

XQ. Exactly. If you believe anything concerning the Simpson Mercury Valve Transmitter which you have not determined yourself from your own observations of the sample here, your belief is founded on information given to you by the representatives of the Marconi Company, is that right? A. No, it is not.

XQ. What otherwise? A. From my experience with

9142

other transmitting sets, including my own, I believe that other Simpson sets characterized by the same arrangement of spark gap, coils and so forth would probably operate in exactly the same way.

XQ. And what are the names of those various persons representing the Marconi Company with whom you have talked concerning this Simpson transmitter? A. Mr. Waterman and Mr. Weagant and Mr. Betts.

XQ. And Mr. Shoemaker? A. Mr. Shoemaker, yes; excuse me.

XQ. Well, how much have you seen of Mr. Betts in
9143 this connection? A. I first saw Mr. Betts on the morning of July 3rd, I believe.

XQ. What were the differences in the conditions of your tests of the Simpson Transmitter prior to July 3rd as compared with the tests of July 3rd and 4th? A. There is no difference in the transmitting set other than a possible difference in the spark gap. I had not previously opened the spark gap and from the operation of the set I can say that the spark gap was in fair condition previous to July 3rd. I have noted that July 3rd the set was very much more irregular than during the tests which I made previous to July 3rd. Furthermore, the radiation obtainable on July 3rd was less.

9144 XQ. Did any representative of the Marconi Company at any time ever suggest to you that the spark gaps or any other part of the Simpson Mercury Valve Transmitter might not be in the best operating condition? A. No.

XQ. All suggestions or changes of that kind came from you exclusively? A. Entirely so.

XQ. With the view of getting the fairest possible results from the apparatus with which you were provided and to which you had been limited by the Marconi Company when they delivered the apparatus to you? A. Yes.

 9145

XQ. Before testing this sample of Simpson Transmitter did you believe it possible that the action in the converting trigger circuit might be as good as in that of the Chaffee Transmitter? A. I was entirely open-minded in the question and before testing the transmitter I thought it might be possible that such might be the case. After seeing the apparatus and connections I thought it improbable that such was the case, but my experiments were conducted entirely impartially.

XQ. That is, as I understand you, it is not at all a condition impossible of attainment to have in the spark circuit a single half oscillation? A. It is not impossible. 9146

XQ. Or radio telegraphic transmitter? A. No.

XQ. You are absolutely certain, I take it, that in the Braun tube which you used in the test of the Simpson Transmitter there were no eddy Fucoult currents in the inside plates of the tube? I assume that you have determined there are no such eddy currents? A. I am satisfied they have no effect if they exist, or no appreciable effect.

XQ. Did either you or Professor Pierce receive cash from the Cutting & Washington Company for your interest in your transmitter patent?

Note plaintiff's objection. 9147

A. I decline to answer. May I add with reference to the previous question that I have also used current coils which are perpendicular to the present current coils, which would exclude any possibility of there being any effect from eddy currents in the electrostatic deflecting plate.

XQ. Can you say whether or not the frequency in the dummy antenna used in your Simpson Transmitter tests was variable or not? A. The frequency was variable.

XQ. And did vary during the tests? A. Was variable when manually adjusted.

9148 Dr. Emory L. Chaffee—Cross.

XQ. Didn't vary at all as a result of the brush discharge of the condensers in the circuit? A. The resonance curves taken on the secondary show that to be true.

XQ. And you are satisfied that the tests you made through that brush discharge of the Leyden jars and the dummy antenna had no effect on the electric action in any respect? A. I am satisfied they had no appreciable effect, at least, to alter the conclusions which I have stated.

XQ. What effect may they have had? Did you at any time in the test substitute for those Leyden jars which gave a brush discharge any capacity which did not? A.
9149 I did not, but tests were made on the other waves in which the brush discharge was markedly less, and the results obtained were the same.

XQ. During your Simpson Transmitter test did you in every case make sure that the point where you connected the spark gap to the dummy antenna was a potential node of the dummy antenna? A. I varied the point of contact between the spark gap circuit and the dummy antenna throughout the range possible with the set and adjusted it for the position of best operation. I do not think it is possible to have a potential node within the range through which it is possible to place this junction.

XQ. By that condition which you have just specified
9150 as the "best operation," then you don't mean the point of potential node on the antenna? A. This junction between the spark gap of the circuit and the dummy antenna was not made at a potential node, or at least with that object in view, but the adjustments were always made for the best operation of the set.

XQ. That is what you considered the "best operation" from your point of view? A. Yes; and what is commonly considered best operation.

XQ. Neither Mr. Simpson nor anybody in behalf of the defendant advised you in the matter, I take it? A. No.

XQ. You understand, do you not, that the plaintiff refused the defendant's offer to make *inter partes* tests of the Simpson Transmitter, but that the plaintiff insisted upon having them *ex parte?* A. I know nothing about that.

XQ. No representative of the Marconi Company at any time heretofore has informed you that that was the fact? A. No.

XQ. That is the fact. What were you informed as to the situation in respect to the desirability of having these tests? A. I was informed by the Marconi Company that certain tests had been made in Washington University- 9152
sity to determine the mode of operation of the set. I was informed that the Marconi representatives did not agree with the results of that test, and that they were interested in finding out the facts. They very clearly stated to me that they were primarily interested in the facts about the case, and that the object in bringing the apparatus East and the object of my tests were to disclose those facts.

XQ. You were given to understand then that the situation was that in order that the true facts might be adduced in the case it was necessary for the plaintiff to have these tests made? A. Yes.

XQ. No represenative of the plaintiff has ever informed you of the fact which I now state, that when the plain- 9153
tiff expressed its dissatisfaction with the defendant's first Braun tube tests in Seattle, the defendant's attorney proposed to the plaintiff's attorney that both sides have an *inter partes* test of this Simpson Mercury Valve Transmitter with a Braun tube so that the facts could be agreed upon and determined in the most fair way?

By Mr. Peters: I was not present at Seattle and am not in a position to testify as my opponent is doing regarding proceedings there. I do know, however, that defendants were invited to adjust

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the set here in proper condition and that they have declined to do so.

XQ. I would like an answer to the question, and as to Mr. Peters' statements, with which I do not agree, we will prove the facts by competent witnesses in that regard. You may answer it, Dr. Chaffee.

Question read.

A. I know nothing about that.

XQ. No representative of the Marconi Company has ever informed you that before initiating these tests by
9155 you the plaintiff declined and refused the defendant's offer for *inter partes* test? A. I know nothing about it. Nobody has told me anything concerning that.

XQ. That is the first time you ever heard anything of that kind? A. Yes.

XQ. In connection with your tests of the Simpson transmitter have you ever tried any kind of rectifying valve in series in converting trigger circuit? A. I have not.

XQ. Can you state of your own knowledge that during your tests of July 3rd and 4th there were no partial discharges occurring in the converting trigger circuit? A. What do you mean by "partial discharges"?

9156 XQ. Anything more than normal corresponding with the generator. A. None of my tests showed the presence of any partial discharges.

XQ. Can you state of your own knowledge whether during any of your tests on July 3rd and 4th the note was absolutely clear tone? A. At certain times I observed the note to be quite good, but I think under the best conditions I have observed a clearer tone than I observed on July 4th, although as I have stated the purity of the tone has made no diffeernce in the appearance of the figure shown in the Braun tube except in clearness.

XQ. Can you produce any photographic record taken of the images on the Braun tube screen in connection with the Simpson Mercury Valve Transmitter on occasions when of your own knowledge the note was perfectly clear? A. I can't say whether any of those photographs were taken when the tone was clearest. I am sure that some of the photographs were taken when the tone was very good.

XQ. Just what work was it that Mr. Washington did in the laboratory here in connection with the Simpson Transmitter? A. Mr. Washington merely observed the operation of the transmitter and made wave length meas- 9158
urements of the secondary and primary circuit. He made no Braun tube observations, and as I understand merely observed the operation, having never seen a Simpson Mercury Valve Transmitter.

XQ. In your tests you made various adjustments of the inductances of the Simpson Transmitter, didn't you? A. Yes.

XQ. That is, adjustments independent of the wave change adjustments provided for by the wave change switch? A. Yes.

XQ. By the adjustments you made, these other adjustments, I referred to the leads, to the inductance coils?
A. I made all possible adjustments that were provided 9159
on the apparatus.

XQ. Not only those adjustments which were frequently referred to as operators' adjustments, but also those adjustments made by the engineer who installs the apparatus on shipboard? A. Yes.

XQ. Were you guided in those adjustments which you made by any directions or instructions from the Kilbourne & Clark Company either written or oral? A. No.

XQ. So far as you in your tests were concerned the situation was as if the defendant had refused to give any

9160

information concerning its apparatus? A. I hadn't looked at it from that point of view.

XQ. Well, that is true so far as you are concerned? A. No; if I understand your question correctly.

XQ. Why? Give your reasons for your answer? A. I didn't ask for any instructions.

XQ. You didn't know the defendant stood ready to help you in all ways but was refused by the plaintiff? A. No.

XQ. So far as you were concerned the defendant stood in the position of one concealing all its operations
9161 from you? A. No; I didn't think of it in that light. I thought I was competent to make the adjustments myself. I did know from having read the testimony that it was assumed that there was a node of potential at the junction between the closed circuit and the antenna circuit. I consequently made, in order to comply with those directions, all possible adjustments that I could to attempt to obtain the best operation. Then, as a matter of belief or opinion, you do not agree with Mr. Kolster of the Bureau of Standards as to the best operation of this Simpson Transmitter accompanying the potential node of the antenna? A. I do not.

XQ. And pursuant to that belief on your part, differ-
9162 ing from Mr. Kolster, you in your tests have not followed that connection from the transmitter to the potential node of the antenna, but you followed some other connection —that is right, isn't it? A. I can't understand how there would be a potential node at the point indicated.

XQ. Then you didn't do that thing in your test? A. Didn't do what?

XQ. Connect the potential node with the antenna? A. I did not; no, but I made all possible adjustments of that point and the position at which I left it is, I understand, the position at which the set is often used.

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XQ. Well, what are your reasons for differing from Mr. Kolster in that regard?

Objected to on the ground that the witness does not know what Mr. Kolster has testified.

By Mr. Farnsworth: He has said he read the testimony.

A. I have not read anything relative to Mr. Kolster's statement relative to the position of the potential node.

XQ. Anyhow, state why you disagree with Mr. Kolster as to that potential node?

9164

Same objection.

A. In my previous answer in which I said I did not agree with Mr. Kolster, I assumed that Mr. Kolster's statement was that the potential node was at the junction between the sparking circuit and the antenna circuit and that this junction is ordinarily about one-half turn or three-quarters of a turn from the junction of the spiral and the condenser. I understand that this potential node is the potential node due to the free oscillations of the antenna circuit. I cannot understand how a potential node can exist at this point, if the transmitting set is exciting
an antenna and the other end of the condenser C is 9165
grounded.

Adjourned to Friday, July 7; at 10 A. M.

9166 Dr. Emory L. Chaffee—Cross.

CAMBRIDGE, MASS., JULY 7, 1916.

MET PURSUANT TO ADJOURNMENT.

PRESENT: COUNSEL AS BEFORE.

CROSS EXAMINATION OF DR. CHAFFEE CONTINUED.

XQ. Will you kindly explain how in the case of a transmitter used as normally with a commercial radio
9167 telegraph antenna you would or could realize the conditions of that test of yours with the Simpson Transmitter in which you removed the wires xx of your Chaffee diagram 1 from point h to point i? A. If the set were used normally with a radiating antenna the connection to the ground at point i would have been equivalent to the shifting of the lead xx to the point i, as was done in the case of the test.

XQ. Do you distinguish between the words "impulse" and "impact" as characterizing radio telegraph transmitters? A. I do not distinguish between those two terms.

XQ. What do the words "impulse" or "impact"
9168 mean in that connection, as you understand it? A. I understand the words "impulse" or "impact" to mean that the primary circuit is discharging with a single current loop in one direction.

XQ. By the expression "single current loop", you mean one half oscillation, do you? A. The single current loop may have a shape similar to a half oscillation, but the single current loop in some cases may be quite different in shape from a half oscillation taken from a conventional wave train.

XQ. In any case what you have in mind is a single unidirectional impulse as characterizing what you term

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an "impulse" or "impact" transmitter? A. The term "impulse" or "impact" as I have used it implies simply a unidirectional discharge and carries with it no stipulation as to its shape.

XQ. As you have defined an "impulse" or "impact" transmitter in that sense, so far as you know is there any impulse or impact radio transmitter on the market save only your own Chaffee Gap Transmitter?

> Objected to as immaterial and improper cross examination.

A. I don't know. 9170

XQ. What was your object in respect to the desirability in designing your Chaffee Gap Transmitter so that it would have a limited time of activity in the spark gap circuit?

> Same objection. Witness is advised that he need not answer questions regarding his own apparatus unless he so elects or is instructed by the Court.

XQ. Dr. Chaffee, I am not asking you about your apparatus, I am asking you about what object you had in mind in attempting to do that, irrespective of what the 9171
apparatus itself is or what it does. My question is limited to your object.

> Objection and advice repeated. Simply use your own judgment about that, Doctor.

A. The Chaffee Gap System was primarily designed not for commercial work, but as a means of obtaining the results in a particular research. The fact that the primary circuit executed single current loops is a result, and partly unexpected result in research.

9172 Dr. Emory L. Chaffee—Cross.

XQ. Then you didn't contemplate that there was any advantage in anywise in having an impulse or impact radio transmitter as distinguished from any other kind of radio transmitter, that is right? A. In the particular research to which I refer it was necessary to obtain continuous free oscillations of a secondary circuit. The impulsive action of the primary circuit of the Chaffee Gap System was most successful in obtaining this result.

XQ. And I take it that also nothing was contemplated as to any commercial advantage in having an impulse or impact radio transmitter when the patent for your said
9173 Chaffee Gap Radio Transmitter was purchased by the Cutting & Washington Co., that is right also? A. I will decline to answer that.

XQ. Mr. Cutting of the Cutting & Washington Company was here present during your cross examination yesterday afternoon? A. He was.

XQ. Do you know why? A. I was not informed why.

XQ. Haven't any idea why, have you? A. I suppose for mere interest. I am certain that he knew nothing about the fact that his name had been or was to be mentioned in any connection with the testimony.

XQ. Do you think that a fair test can be made of the operation of a radio telegraph transmitter by supplying
9174 the energy from it to a deflecting coil outside the Braun tube? A. I don't understand your question.

XQ. Well, just answer the best you can, and if through my unfortunate ignorance I can't express it properly, perhaps you will be kind enough to help me out. A. I consider the use of a current deflecting coil properly connected to a radio transmitter is a proper means of investigating the action of the transmitter.

XQ. The deflecting coil having what relation to the Braun tube? A. The deflecting coil being outside the Braun tube and properly shielded electrostatically from the inside of the tube.

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XQ. And what is such proper shielding, in your opinion? A. Proper shielding consists in surrounding the tube inside the current deflecting coils by conducting netting in which no eddy currents will be introduced, but which will shield the interior from the variations in potential which exist between different parts of the current deflecting coil.

XQ. And what do you consider the proper connections from such a deflecting coil to the radio transmitter? A. The deflecting coils may be made to carry the whole current being investigated or may be made to carry a portion of the current by shunting the coil about a conductor carrying the main part of the current. 9176

XQ. How may one be sure that there are no eddy currents in the apparatus of the Braun tube you have referred to shortly previously? A. Eddy currents in what part of the apparatus?

XQ. The one you referred to above a moment ago. A. There is provided in the shielding no possible path for eddy currents which would in any way cause a deflection of the spot.

XQ. How did you know that you could not have so proportioned your dummy antenna in your Simpson Transmitter tests as to have made it possible to find a potential node within the limits of possible adjustments? 9177 A. If the dummy antenna was oscillating in its fundamental or lowest natural frequency, as it was, and if the point h was grounded, there would be from theoretical considerations no potential node on the spiral s within the range of adjustment furnished by the apparatus.

XQ. Will you please make a diagram illustrating the oscillations in the spark gap circuit of the oscillations in the antenna circuit in the case of your Chaffee Gap Transmitter in the case of 1000 sparks per second by alternating current excitation? A. The transmitter is not designed to so operate.

9178 Dr. Emory L. Chaffee—Cross.

XQ. You cannot operate your Chaffee Gap Radio Telegraph Transmitter by alternating current supply and at 1000 spark frequency per second? A. I have not done so.

XQ. And you think it is impossible to do so? A. It is not impossible.

XQ. Well, if it is not impossible, then please make the diagram.

Last objection and advice regarding Dr. Chaffee's apparatus is repeated.

9179 A. I decline to answer.

XQ. Is Mr. Cutting of the Cutting & Washington Company here again this morning? A. He is.

XQ. Do you understand he is going to be a witness for the Marconi Company? A. He is not, to my knowledge.

XQ. Can you say, Doctor. whether or not any of your various forms of tests of the Simpson Mercury Valve Transmitter was suggested by either Mr. Weagant or Mr. Waterman of the Marconi Company? A. I believe all the tests were of my own design and carried out according to my own ideas.

9180 XQ. Since we adjourned last evening at six o'clock, have you spent any time in conference in respect of your present deposition? A. There has been nothing said in conference which has in any way infuenced me in my present deposition.

Question repeated.

A. A few moments last evening.

XQ. Who was present? A. Mr. Waterman, Professor Morecroft and Professor Coffin and I believe Mr. Peters part of the time.

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The defendant now enters objection to each and all the exhibits introduced by plaintiff in connection with the present deposition, including Dr. Chaffee's diagrams and photographs; and defendant also now enters objection to all parts of the present deposition relating to such exhibits; and defendant now moves that all the same be stricken from the record on the general ground that each and all the same have been shown by the deposition to be incompetent and immaterial and the question sought by plaintiff to be established, namely, the mode of operation of defendant's 9182
Simpson Mercury Valve Transmitter when used in commerce and in the then customary operating conditions. Defendant admits that the present deposition and the exhibits do not disprove defendant's statements as to the operation of its Simpson Mercury Valve Transmitter, but contends and will move before His Honor, Judge Neterer, that these alleged proofs of plaintiffs do not affirmatively establish anything different from defendant's previous proofs.

(Signed), E. L. Chaffee.

REDIRECT EXAMINATION BY MR. PETERS. 9183

RDQ. Defendant's experts have represented to this Court that a dummy antenna is a proper substitute for a commercial antenna for the purpose of making Braun tube tests on their apparatus. I suppose you agree with them? A. I do.

RDQ. And the dummy antenna you used was a proper antenna for the purpose? A. I considered it so.

RDQ. The defendant's expert, Mr. Ford Greaves, has testified that he did considerable work under your di-

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rection on Braun tubes. Do you remember him in this connection? A. I remember that Mr. Greaves did some work with Professor Pierce on Braun tubes, but I do not remember any work which Mr. Greaves did with me.

RDQ. What, if anything, did you observe during your tests regarding the operative characteristics of the 500-cycle generator which you employed? A. I did not observe anything abnormal in the operation of the 500-cycle generator which I used, and consider it a fair representative of commercial 500-cycle generators used for this purpose.

9185 RDQ. From your extended tests, on this Simpson mercury valve transmitter, what can you say as to the possibility of operating it with a single impulse in its primary circuit? A. I believe that it is impossible to obtain, with any possible adjustment of the apparatus, a single impulse in the primary circuit.

RDQ. What was your object in using two discharges per half cycle in some of the tests? A. Early in the tests, I observed that in such tests which were used to prove the existence of oscillations, in the primary circuit, the characteristics of the figure, as seen in the Braun tube were identical in every respect, both when one discharge per half cycle of charging current or more discharges
9186 per half cycle of charging current were used.

The brilliancy, however, of the figure, was greater when more discharges were used, consequently photographs were often taken when a greater number of discharges were used, in order more easily to obtain a clear photograph. I have shown, however, photographs in which a single discharge took place in the primary circuit per half circle of charging current, and these photographs will be seen to be similar to the photographs taken with a greater number of discharges.

RDQ. On cross-examination, you stated that you had

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used current deflecting coils disposed at right angles to the Braun tube deflecting plates. You did not explain this statement of fact. Will you please do so? A. Objection was made to the results obtained with the current coils parallel to the deflecting plates, on the ground that there might be, possibly, eddy currents in those deflecting plates, and that these eddy currents might alter the results obtained. To show that the eddy currents have no effect, I made use of current coils, which were perpendicular to the plane of the generating plates, and the figure obtained in the Braun tube was identical with
the figure obtained with the coils, as used in the test. 9188

RDQ. Please explain a little more fully the means you employed for obtaining the time axis on your photographs. Interpret this axis as it appears on the photographs. A. The connections of the electrostatic plates, which give the time axis to the photographs are shown in diagram 1. The condenser "C2" had a capacity of .002 micro-farads. The resistance "R2" was of the order of 2400 ohms. Resistance "R1" was measured, and found to be 9000 ohms. The resistance "R1" is so large that the circuit containing "R1" "L1" and the capacity "C2" is non-oscillatory.

The condenser "C2" is charged by a low frequency
charging current, received from the transformer, and 9189
increases in potential simultaneously with the increase in potential of the condenser "C". At the moment of discharge of condenser "C" through the primary circuit, the condenser "C2" begins to discharge through the resistance "R2". The potential of "C2" and, consequently, the potential of the electrostatic plates "P" begins to drop, and the spot of light moves across the screen of the tube. Since the condenser "C2" discharges more rapidly at first than later in its discharge, equal distances on the deflection in the tube do not represent

equal time intervals. Consequently, the figures showing the wave train in the Braun tube is not the same in exact shape as would be obtained if equal distances on the time axis represented equal increments of time as in the conventional method of representation.

RDQ. Please state whether, after you opened the spark gaps on the evening of July 3 for the purpose of cleaning and repairing them, you showed the gaps to Mr. Simpson and to defendant's experts before replacing them in the apparatus. Explain the facts in this regard. A. The defendant's experts were not present when the
9191 gaps were first opened.

RDQ. Why? To save another question, state why? A. It was not decided to open the gaps until after the conclusion of the tests on July 3, and after the defendant's experts had left the laboratory. The gaps were repaired, and the distances between the sparking surfaces adjusted to what we understood to be the proper distance.

On the morning of July 4, after a part of these tests had been conducted, the gaps were reopened, and shown to the experts of the defendant. Mr. Simpson then stated that the normal distance between the sparking surfaces was six mills, which was the distance for which we had
9192 adjusted the gaps. Mr. Simpson further admitted that it was normal practice to open the gaps and clean them as we had done.

RDQ. The gaps, as I understand, were sealed air tight before being reinserted in the apparatus and used? A. They were.

RDQ. Have the gaps been used or disturbed in any way since your tests of July 4? A. They have not.

RDQ. Will you please now open them, in the presence of defendant's counsel and experts and ascertain their present condition? A. I will now do so.

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RDQ. Have you now opened the gaps in the presence of defendant's experts? If so, please state their condition. A. I have opened the gaps in the presence of the experts, and find that they are in excellent condition. I have closed the gaps, and sealed them, writing my name, and the words "Sealed July 7" on each seal.

RDQ. Did the opposite plates of each gap have the same appearance? A. The two plates had exactly the same appearance, showing no unsymmetry, so far as I could detect.

RDQ. Defendant has represented to this Court that it used its transmitter as illustrated in defendant's ex- 9194
hibit "Simpson drawing, F. G. S. 2". (See Vol. 2, p. 1080.) Assuming that the generator "D" shown in this drawing is a 500-cycle generator, provided with variable reactance, how does the Simpson transmitter as tested by you as to connections and arrangements of parts agree with this drawing? A. The connections of the transmitter used by me in the tests are the same as those shown on drawing "F. G. S. 2", with the exception of the substitution of a dummy antenna for the aerial "2".

RDQ. In the *ex parte* tests conducted by the defendant at Washington University, it appears that a dummy
antenna was used, and was connected as shown in defendant's Exhibit "F. G. S. 7". (See Vol. 2, p. 1131.) How did the dummy antenna you used, as to connection and arrangement, compare with the one shown in this drawing? A. The arrangement of the dummy antenna in "F. G. S. 7" is the same as the dummy antenna which I used, with the exception that no concentrated inductance is shown in "F. G. S. 7", to correspond with the variable inductance I have shown on "Chaffee diagram 1". 9195

RDQ. Did the small variable inductance which you used in your dummy antenna make a difference in character or mode of operation from the dummy antenna shown in "F. G. S. 7"? A. The concentrated variable inductance which I have used would be entirely equivalent, so far as the operation of the set is concerned, to the inductance of long leads, as I understand was used in the Washington test.

(Redirect examination closed.)

By Mr. Farnsworth. Defendant offers in evidence as defendant's exhibits, these five pairs of
9197 halves of spark gaps which Dr. Chaffee has just testified he has sealed under date of July 7, and defendant requests that those spark gaps be forwarded by the notary examiner to the clerk of the Federal Court at Seattle, Washington, in order that Judge Neterer may personally see the condition of said spark gaps.

By Mr. Peters: The entire Simpson mercury valve transmitter will be marked and forwarded to Seattle.

By Mr. Farnsworth: I direct the notary examiner to mark the said spark gaps in evidence,
9198 as defendant's exhibits, to be forwarded accordingly.

(Deposition closed.)

(Signed) E. L. Chaffee.

AFTER RECESS.

Whereupon JOHN HAROLD MORECROFT, a witness called on behalf of the plaintiff, having been first duly sworn, testified as follows:—

DIRECT EXAMINATION BY MR. PETERS.

Q. Please state your name, residence and occupation. A. John Harold Morecroft; Palisade, New Jersey; teacher.

Q. What experience and training have you had which 9200
qualifies you to test wireless telegraph apparatus, particularly by the Braun tube methods? A. I was graduated from Syracuse University with the degree of Electrical Engineer in 1904. I was then foreman of a machine shop for a year, re-entered Syracuse University and took the degree of Bachelor of Science and Physics in 1907. Was instructor in electrical engineering at Pratt Institute, Brooklyn, 1907 to 1909. Since 1909 I have been at Columbia University in the positions of instructor, assistant professor and associate professor of electrical engineering, which rank I now hold. From 1907 until 1913 I was virtually the research assistant of Professor
Pupin, Columbia University, and since that time have 9201
carried on various experiments by myself. I am the author of three published books on electrical engineering topics. I have published various articles on phases of the theory and practice of electrical engineering. My experience with Braun tubes dates from 1914, at which time I endeavored to do some research work in radio telegraphy by the aid of the ordinary type of Braun tube. During the course of these preliminary tests I was struck by the inadequacy of electrostatic deflections of the cathode stream produced by outside deflecting plates, during

the last year or 18 months possibly, I, with one of my colleagues, having been working on a special form of Braun tube designed for the photographing of the high frequency currents occurring in radio telegraphic circuits. While developing this special form of tube I have investigated very many of the peculiarities of these tubes, and feel that I am qualified to state as to whether or not an experiment carried out with Braun tubes, the conditions of said experiment being fully outlined for me, are reliable and safe enough to draw logical conclusions therefrom. My work in radio telegraphy may be summed
9203 up by stating that I have charge of all instruction work in radio telegraphy at Columbia University, the institution selected by the United States Navy to be the place at which naval officers intending to specialize in radio telegraphy should study. I have during the past three years carried out both the theoretical and laboratory training of such men.

Q. Were you present at the Cruft Laboratory, Harvard University, on July 3rd and 4th during the tests conducted by Dr. Chaffee on the Simpson Mercury Valve Transmitter?

This and all other questions relating to the
9204 tests conducted in the Cruft Laboratory objected to as incompetent and immaterial, for the reason that the radio apparatus tested was not the apparatus of the defendant; this objection to apply to all questions of like import.

A. I was present July 3rd and 4th at the Cruft Laboratory of Harvard University witnessing certain tests carried out by Dr. Chaffee of that institution on a radio transmitting set in which a mercury valve is used, the panel board of which carried a name plate indicating that

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the apparatus is the Simpson Mercury Valve Radio Transmitting Apparatus.

Q. Did you observe carefully the methods used by Dr. Chaffee in conducting his tests? A. I followed out carefully the connections and operation of the apparatus which Dr. Chaffee used in making these tests and regard the manipulation and carrying out of the test as very ingenious. I believe that the work was carried out thoroughly from the impartial standpoint which every scientist is supposed to have in attacking any problem in which the truth is to be discovered.

Q. Were the methods used by Dr. Chaffee fair, ac- 9206
curate and proper methods? A. Yes.

Q. What opportunity did you have during the tests to observe the results had? A. I was a very close observer of the screen of the Braun tube on which the figure is drawn, which figure represents in this experiment the form of the current actuating the motion of the cathode ray.

Q. What is your conclusion as to the oscillatory character of the primary circuit current in that Simpson Mercury Valve Transmitter based upon the tests conducted by Dr. Chaffee in your presence? A. The conclusion to which I am forced by experimental evidence is
that under all the conditions existing during which I 9207
was making observations the current in the closed primary circuit of this apparatus was oscillatory in character.

Q. Will you please make a diagrammatic drawing showing the character of current in the primary circuit as you saw it on the screen of the Braun tube during Dr. Chaffee's tests? A. Dr. Chaffee used two arrangements of circuits in carrying out his tests, one of which gave what we call a straight line deflection to the cathode ray, the other arrangement of which served to so change the

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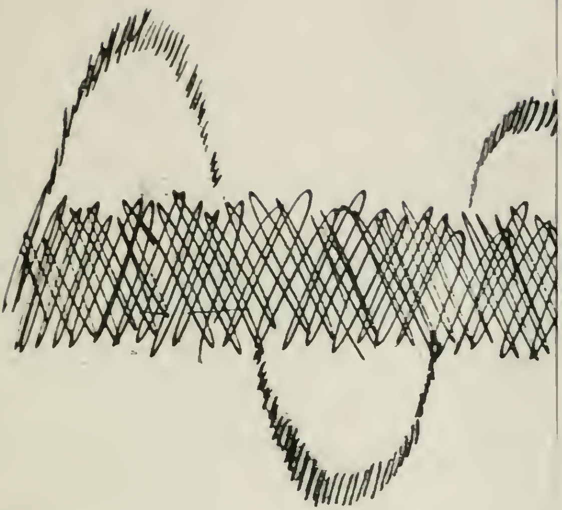
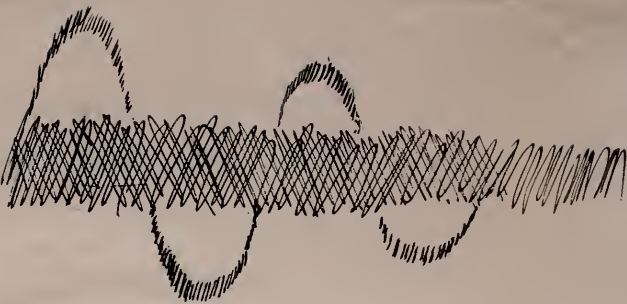
figure as to depict the current more or less in the form of a sine wave, as we are accustomed to seeing such currents represented. I will sketch the form of the current as shown by the Braun tube with the latter arrangement of circuits. (Inserted opposite with Diagram No. 2.) I have so represented in this sketch, as nearly as I can draw it, the form of the path taken by the cathode ray, which represents the oscillatory character of the current in the closed primary circuit. It is not possible to represent exactly in such a manner the exact appearance of such a phenomenon, it being especially
9209 difficult to correctly represent the relative strengths of the broad central band with respect to the sinusoidal path of the cathode ray, so that the sketch I have drawn does not give relative intensities of the broad zero line as it occurred on the screen and the sine wave of current. On the screen the broad central band always appeared with a relatively very much greater intensity than the wavy line. This is to be at once expected when one appreciates the fact that the spot of light in traveling over the wavy line moves approximately ten to twenty miles a second, and that it is going over this path for a time equal to about 1/1000th of the time (for some of the conditions)
9210 and perhaps 1/500th of the time for other conditions of the time during which it travels over the central band. I will label this sketch Morecroft Diagram 1. While I have shown on this sketch only two complete waves, there were at times two and one-half visible on the screen.

Q. Will you also make a diagram drawing showing the character of the current in the antenna circuit of defendant's Simpson Valve transmitter as you saw it during Dr. Chaffee's tests? A. The form of current as shown on the screen of the Braun tube when the magnetic deflecting coils were connected across a piece of the dummy antenna circuit was as I will endeavor to sketch in Diagram 2. (Makes sketch.) (See p. 3071).





Morecroft diagram 1

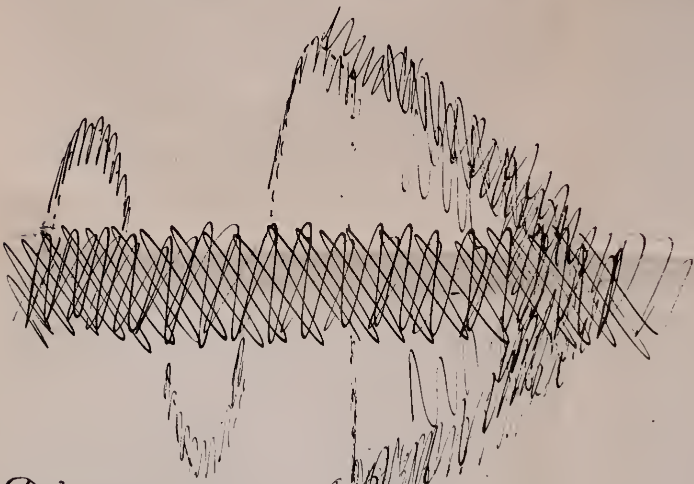


Diagram 2

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This I have done and I wish to say further in regard to this diagram that it is impossible to represent the relative intensities here also, of the central broad band and of the much less distinctly defined sine waves, or approximately sine waves, as observed on the screen. I wish also to add with respect to these two diagrams that the scales are not quite the same, a given amount of time being shown as a greater distance in the top diagram than in the lower diagram. I also wish to call attention to the fact that no conclusions can be drawn from these two diagrams regarding the phases of current in
the closed circuit and in the antenna circuit because a 9218
mere interchange of the two leads coming to the magnetic deflecting coils will reverse the time phases of the curve.

Q. Will you, by reference to your diagrams, briefly describe the oscillations in the two circuits. A. The oscillations in the closed primary circuit were quite apparently of the form of an approximately sine wave dying away at some rate which cannot be predicted from this diagram and consisting of at least two complete cycles and how many more I do not know. The current in the antenna circuit was quite evidently an oscillatory current
which built up very rapidly until it reached a certain 9219
maximum and from that time on died away. The dying away of this antenna current is not directly visible, either on the screen or on the sketch I have made because of the fact that the scheme for obtaining the so-called time axis of this diagram was such that the oscillations were crowded together so much towards the end that they practically overlapped and the only thing which was visible on the screen was what we call the envelope of the curve or a line connecting the successive maximum values of the oscillatory current.

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By Mr. Peters: I offer in evidence the sheet of drawing made by the witness and the same is marked "Plaintiff's Exhibit Morecroft Diagrams 1 and 2". (Plaintiff's Ex. No. 67B.)

By Mr. Farnsworth: The objection above made to the testimony is repeated as to this exhibit and is to be extended to all other exhibits of like import. That is, the objection of incompetency and immaterialty with respect to the apparatus under test not being defendant's apparatus.

9221 Q. Was the artificial antenna employed in the tests of July 3 and 4 in your opinion a proper substitute for a working antenna for test purposes? A. I can see no reason whatsoever why the use of such a dummy antenna is not perfectly justifiable in making these tests, the question in mind being to ascertain whether not the current in the primary circuit is oscillatory or not.

It may be well to remark here that from experience in Government testing I am able to state that practically all Government wireless sets have their acceptance tests made on dummy antennæ similarly constituted to that which Dr. Chaffee used. The capacity which he used in his dummy, namely, .001 micro-farads, approximately,
9222 there being in the antenna circuit a resistance in the neighborhood of 6 or 7 ohms, it seems to me that such a dummy antenna is a perfectly fair substitute for the ordinary working antenna such as is used on the average merchant ship.

Q. You were present during Dr. Chaffee's testimony given here? A. To the best of my recollection I was present during all of the direct examination.

Q. It appears that Dr. Chaffee during the tests opened the spark gaps on a number of occasions. Did you inspect the gaps when they were opened? A. To the best

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of my knowledge I was present every time the gaps were opened and each time I made a careful inspection of the gaps.

Q. What can you say as to the appearance of the gaps; were they colored equally on both plates?

Objected to as leading, as many other of the direct questions are.

A. Upon each of my inspections the coloring of the plates on both sides of the gap was so nearly alike that I could make no distinction between them whatsoever. 9224

Q. The defendant herein made certain *ex parte* tests of what was alleged to be their apparatus at Washington University in Seattle and it appears that during those tests the Braun tube was used and the electrostatic deflecting plates and Braun tube were outside of the tube and were connected directly across the spark gap of the transmitter. What can you say as to conclusions based on such a test, regarding oscillations in the primary circuit? A. The deflections of a cathode ray due to electrostatic deflecting plates on the outside of the tube, said plates being connected across the spark gap in the primary circuit are no indication whatsoever of the oscillatory or non oscillatory character of the currents in the 9225 primary circuit. This is due to two reasons which I will give herewith.

First, when outside plates are used in the Braun tube the deflection of the cathode ray is not at all proportional to the voltage impressed on these two plates. Thus, for example, if the position of the spot on the fluorescent screen is noted when there is no voltage impressed on these plates and then, let us say, 2000 volts are impressed on the plates and maintained, the spot may still be at zero and in the average Braun tube always will be found there. I could give several more illustrations

based on experimental evidence which show that the deflection of this cathode ray does not follow at all the voltage impressed on the outside plates. Now, the second reason why the image on the fluorescent screen of the Braun tube in the Washington University tests is no indication of the oscillatory character is more elementary and more easily understood than the first. Let us suppose that there is no error due to outside plates being used, that is, we will suppose that if outside plates are used the deflection of the spot will be proportional to the voltage impressed on these plates. Now, before, the
9227 spark gap breaks down the voltage on this gap, in such a set as I have seen in the Cruft Laboratory, is of the order of three or four thousand volts, I suspect. Hence, before this spark gap discharges the spot will have moved over a distance on the screen proportional to three or four thousand volts. It might, for instance, be, let us say, an inch of deflection. Now, when this spark gap breaks down, its high insulation at once disappears and the only difference of potential or difference in voltage which can occur across this spark gap is due to the so-called IR drop through the gap. As to just how much this IR drop is it is difficult to say, but in my judgment
9228 will be of the order of a few volts. Let us grant then, for the moment, that there are oscillations in this circuit and grant, as I have previously stated, that the deflections of the outside plates are correct; we might then expect on the fluorescent screen a deflection of one inch due to the high voltage existing across the spark gap before it breaks down, and after it has broken down and the supposed oscillatory current is taking place the drop across the gap might be, for instance, ten volts. Hence, even though the oscillations were there the spot of light on the fluorescent screen during the oscillatory condition of the circuit would be moving over a space of per-

haps 1/300 of an inch. It will be at once evident that a motion of such a magnitude of this spot in the Braun tube is not at all discernible. This last reason which I have given why the Washington University test photographs are no indication of the actual conditions existing in the circuit is such a one that anyone skilled in the art of using electrical apparatus would not appreciate at once. In resumé, I state therefore that due to the use of outside plates and due to the way in which these outside plates were connected to the circuit these photographs are of no value whatsoever.

Q. Did you observe with respect to the Simpson Mercury Valve set whether or not it was necessary to carry 9230 out the same tuning and coupling adjustments of the primary circuit with respect to the secondary circuit as is necessary on other sets with which you are familiar in order to get maximum radiation? A. While I myself did not actually manipulate this set, I watched the manipulations which Dr. Chaffee carried out and noted the result on the ammeter roughly indicating the possible radiation from the antenna circuit, and my judgment is that this set requires approximately the same adjustment for tuning of the two separate circuits and for getting the right coupling between the two. In fact, in so far as tuning and coupling adjustments are concerned I 9231 cannot see any differences between this set and other sets on which I have worked.

Q. And is the set provided with means whereby an operator may readily make these adjustments? A. The adjustments which I have in mind cannot be carried out by an operator unless he changes the position of certain of the taps, etc. except as these conditions are properly fulfilled when the set is changed from one wave to another by means of the wave change switch. In short, the adjustments which I had in mind when answering the

9232 previous question were what I believe are called installers' adjustments, not operators' adjustments.

Q. I believe you have stated that you heard Dr. Chaffee's testimony on direct examination. Do you or do you not agree with the conclusions expressed by him regarding the defendant's apparatus and the tests?

> Objected to as very vague and indefinite. Do you mean Mr. Morecroft to give bodily in his deposition Dr. Chaffee's depostion in toto and say that he agrees with it or not?
>
> By Mr. Peters: The question speaks for itself.
>
> 9233 By Mr. Farnsworth: Then there is no need of asking Mr. Morecroft any further questions. Just stop with this question and close the deposition.
>
> Mr. Peters: I will.

A. In so far as Dr. Chaffee's tests indicated the presence of oscillations in the primary circuit under all conditions of the set in which I saw it operating I agree with Dr. Chaffee's conclusions. In fact, from the experimental evidence which I had, nothing but agreement is possible.

9234 Direct Examination Closed.

CROSS EXAMINATION.

Questions by Mr. Farnsworth.

XQ. Please give a list of the papers you have read or published which relate specifically and expressly to subjects of radio telegraphy, specifying the names of the papers and places or reading or publication, or both. You will notice that I said radio telegraphy. A. By the reading of papers I presume is meant the presentation before some society of results of my own tests in radio telegraphy. I have published none.

XQ. And read none, presented none? A. I have presented none.

XQ. Nor published any. A. Nor published any. If you will read my answer you will see that I said published, before.

XQ. Have you ever testified before in patent litigation? A. I have never before testified in patent litigation.

XQ. Will you calculate the number of the antenna oscillations in the dummy attenna employed in Dr. Chaf- 9236
fee's tests of July 3 and 4? A. I am afraid that such calculation would not be a satisfactory answer to the question, as there exists an infinite number of such oscillations and the calculation of an infinite number is not worth while in ordinary calculations.

XQ. You couldn't give me the number as ordinarily spoken of as the result of calculation of that antenna? A. If you will tell me what you mean by ordinarily spoken of, I perhaps might calculate them for you.

XQ. What do you think I mean? What is the way in which these oscillations are usually spoken of? Don't you know? A. Yes, my dear man, I know. 9237

XQ. Well, say so then. A. The number of oscillations in such a circuit is, as I have said before, infinite. A system which has been started in oscillation, having no spark gap therein, will continue in oscillation ever afterward. If the question which is being asked is presented from a so-called practical standpoint it is necessary to assume that the wave train is supposed to end after the amplitude of the oscillation has fallen to a definite fraction of its first maximum. If you care to state the question in such a form that I know where you wish to consider the wave train ended, I will calculate

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for you how many oscillations exist in a certain supposed antenna.

Q. (Question read). A. Assuming a decrement of 1/10 and assuming the wave train is ended when the amplitude of the current has fallen to 1% of its initial value, there will be in the neighborhood of 50 or 60 oscillations, the exact number not coming from the formula I used without the use of a slide rule.

XQ. How many oscillations would there be in that dummy antenna of Dr. Chaffee's if it had a decrement .02? A. About 240.

9239 XQ. What was the decrement of that dummy antenna of Dr. Chaffee's in his test of July 3rd and 4th? A. The resistance of the dummy which Dr. Chaffee used was six ohms. The resistanace of the loading coil connections, series condenser, and so forth of the antenna circuit I have assumed to be two ohms, which is larger than will exist unless there was some imperfect connection, or something of that kind. Therefore, assuming the total resistance of the antenna circuit to be eight ohms, the total inductance of this antenna circuit was 100 microhenries and the decrement would be .08.

XQ. What was the number of antenna oscillations in Dr. Chaffee's dummy antenna, assuming the decrement
9240 you have last given of .08? A. The number of oscillations occurring before the wave train dies to one per cent. of its original value in this case, that is, wave length 600 meters, decrement .08, is 54.

XQ. Please make a diagram No. 3 similar to your diagrams 1 and 2 in respect generally of showing the oscillations in the spark circuit and antenna respectively, but make diagram 3 to scale as to oscillations both in the spark circuit and dummy antenna circuit of Dr. Chaffee, the numbers of oscillations in the case of both the spark circuit and the antenna circuit being those you

3081

lant's Exhibit No. 53.

losed circuit
here an indefinite number of
times appeared two and
one half.

l in antenna circuit.

08 there are about 54 waves
rams before the
ave has fallen to 1%
e

ft) Diagram 3

pinion of the form of
l oscillating circuit (above)
antenna circuit (below)
Simpson's Mercury Valve

have testified about and including the 54 oscillations in the dummy antenna. A. I ask for further information on that question. Do you wish me to draw that diagram as it would appear on the Braun tube screen or as is ordinarily depicted with a uniform time axis?

XQ. The latter. A. I have so shown and mark this diagram Morecroft Diagram No. 3 (reproduced opposite).

XQ. Will you add there below "Morecroft Diagram No. 3" something about conditions in the Cruft Laboratory July 3rd and 4th, whatever the fact may be, with dummy antenna? A. I have so done.

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> The diagram 3 produced by the witness is offered as Defendant's Exhibit Morecroft Diagram No. 3. (Deft's. Ex. No. 53.)

XQ. Does or does not the variation of the antenna inductance used in the Chaffee dummy antenna change the potential distribution in the antenna and alter the position of the potential node or nodes existing in the dummy antenna? A. Well, it may be possible that the so-called node of potential in the dummy antenna circuit was affected slightly by the variation in the added inductance of the antenna circuits. It must be borne in mind that this inductance was very small compared to 9249
the inductance already in the antenna circuit, my estimate being about fifteen per cent, and, moreover, through the progress of the tests to the best of my knowledge this small amount of added inductance was changed but very little. If, therefore, we consider the change of total dummy antenna inductance due to the variation of this added inductance, I find the change as carried out in these tests was very small. It is my opinion, therefore, that there was no material shift in the voltage node.

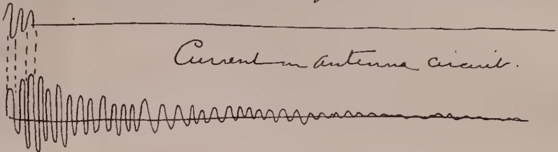
XQ. You think there were nodal points, or was a nodal point on that antenna, do you? A. Meaning by

Defendant's Exhibit No. 53.

Def Ex 53

Current in closed circuit

I have shown here an indefinite number of oscillations as there sometimes appeared two and sometimes two and one half.



Thumb 58,5

For $\delta = .08$ there are about 54 waves in the lower diagrams before the amplitude of the wave has fallen to 1% of its maximum value

Morecroft Diagram 3

J H Morecroft' opinion of the form of current in the closed oscillating circuit (above) and the dummy antenna circuit (below) of the set marked Simpson's Mercury Valve transmitter, tested by Dr E. L. Chaffee witnessed by J H Morecroft in Cruft Laboratory July 3-4-1916.

9250

"node" a point on the antenna where minimum of potential with respect to the ground occurs while the antenna is in a state of oscillation, it is my opinion that such a point may exist, the dummy antenna circuit being grounded.

XQ. Was there any point on that Chaffee dummy antenna zero potential? A. Most assuredly.

XQ. Where? A. Where the dummy antenna circuit connects to the ground.

XQ. But not a zero potential, though, at any other point? I take it, of course, from your last answer that
9251 you mean that there was in that Chaffee dummy antenna no other point or node of zero potential, save the ground connection. A. You have no business to assume any such thing.

XQ. Then please state the fact in full. My questions, you know, doctor, are not limited to specific answers, but an endeavor to find out all the truth. A. Now do you ask me whether there are two points in the dummy antenna circuit at which a node exists?

XQ. Any and all points, if you wish to tell it all. A. Meaning again, as I said before, by "node" a point where the voltage of the antenna system remains continually the same as the earth potential, there was no such point
9252 other than the point which I have previously designated, where the dummy antenna connects to ground.

I wish further to add that I do not believe in any antenna, actual or dummy, that there is a point on the antenna where the voltage continually remains the same as the ground, and I will state briefly my reasons for this answer, as it is easy to see where a superficial examination of the texts written on this subject would lead a novice into error. In the text of my colleague, Professor Zenneck, we find certain diagrams where the voltage of the antenna is supposedly shown in the form of a curve.

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This curve, in this diagram I have in mind crosses the antenna at certain points, and therefore one is led to believe that the author means by this the voltage at this point of the antenna remains continually at ground potential. Such is not my interpretation of his diagram. The question of nodes in an oscillating system is not so simple as one might think by glancing at these diagrams and reading the short descriptions given therewith. When a system such as an antenna is in oscillation, the energy is being continually transferred from one end of the system to the other. At the same time, the energy
is being dissipated from the system. Now, when the en- 9254
ergy is dissipated from one part on the system more rapidly than from another part of the system, the diagrams of Professor Zenneck, to which I have previously referred, do not hold good. There is in this case no point upon the system where the potential remains always at ground potential. As in the dummy antenna circuit which Dr. Chaffee arranged, the energy is dissipated more rapidly from one point of the system than the other, there is no such thing as a real node of potential on this antenna circuit except the ground point. Neither is there any such point where the voltage remains always at zero in an actual antenna.

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Cross Examination Closed.

RE-DIRECT EXAMINATION BY MR. PETERS.

RDQ. You have stated that this is the first time you have ever testified in a patent case. Please state the circumstances under which you have appeared here. A. I was first notified of the case over the telephone by Mr. Weagant, approximately two weeks ago. He asked me, or rather notified me at that time that he was going to

9256 have Mr. Waterman (whom I did not know) call me up over the telephone and ask me to be what is called a "fact witness" at certain tests which Dr. Chaffee was going to carry out by means of the Braun tube at Harvard University. Mr. Waterman at that time expressly told me the thing he was interested in was knowing the facts of the case, and simply wanted me to come because of my experimental experience in general, and tell him whether, as a result of these experiments, it was my opinion whether there were or were not oscillations in the primary circuit of this transmitter.

9257 RDQ. Referring to your diagram Number 3 in which you show an antenna wave train of some fifty oscillations, I believe, can you estimate what portion of the actual energy is radiated by, say, the first four or five of the oscillations in this train? A. Yes, if you so desire, I will calculate it.

RDQ. Please do so. A. I have figured the energy dissipated by the antenna between the time when its maximum current occurs and six oscillations later. So that if we consider the energy at the time when the antenna is at the maximum current and therefore maximum energy and consider the time six oscillations later, I say
9258 that during this time 63% of the energy will have disappeared from the antenna circuit.

RE-CROSS EXAMINATION BY MR. FARNSWORTH

RXQ. Did Mr. Waterman or anybody else representing the Marconi Company ever tell you before you came to Cambridge upon this trip the fact that the Marconi Company had refused the Kilbourne & Clark Company's proposal for *inter partes* tests of the Simpson Mercury Valve Transmitter? A. I was not so informed.

Q. (By Mr. Peters) Would it have made any differ-

 9259

ence in your testimony if you had been informed of said facts? A. It would have made no difference whatsoever.

(Deposition closed.)
(Signed) JOHN HAROLD MORECROFT.

Whereupon JOSEPH GEORGE COFFIN, a witness called on behalf of the plaintiff, having been first duly sworn, testifies as follows:

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DIRECT EXAMINATION.

Questions by Mr. Peters.

Q. State your name, residence and occupation. A. Joseph George Coffin; 382 Wadsworth Avenue, New York City; teacher of physics.

Q. What experience and training have you had which qualifies you to take part in and to testify as to tests of wireless telegraph transmitters, carried out by means of Braun tube apparatus? A. I lived six years in Europe. I mention this, in order to show that, speaking French, German and Italian, I am able without difficulty to follow scientific literature in those languages. I graduated in 9261
1898 from the Massachusetts Institute of Technology, specializing in physics and electricity, experimental and theoretical. The degree I obtained there is Bachelor of Science.

I taught in the Institute for two years. One of those years I was assistant to Professor Cross. In 1900 I received a scholarship allowing me to continue my studies under Dr. A. C. Webster, Clark University.

In 1903 I received from the University the degree of Doctor of Philosophy in Mathemetical and Experimental Physics. From 1902 to 1906 I taught at Clark College.

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In 1906 I went to the College of the City of New York and have been there ever since. I am now Assistant Professor of Physics there.

At Clark University I had charge of research work in various branches of physics. I was Dr. Webster's private assistant for two years, and in my teaching work I have had charge of laboratories and have delivered courses in all branches of physics, such as theoretical electricity, electrical oscillations, conduction of electricity from gases, and other branches of physics.

I am in charge of several laboratories doing work in
9263 various branches of physics.

I am at various times a member of the American Physical Society. I am a member of the Institute of Radio Engineers. I am a Fellow of the American Association for the Advancement of Science and I have kept out of other societies because I didn't want to pay the dues. I am also a member of the American Mathematical Society.

My work has made me familiar with telephones, oscillographs, Braun tubes, and laboratory manipulation of all kinds. I am particularly interested in the reduction of physical observations, the accuracy of observations, and the precautions to be taken to make them reliable; and the
9264 proper way to plot curves.

I have written a book on mathematical physics entitled vector-analysis, which has been translated into French and would have been translated into German, had the war not stopped it. I have written several papers, some of them for the Government, on construction and calculation of standards of self-inductance; on the influence of frequency on the self-inductance of coils; also one on the influence of frequency on the capacity of absolute condensers.

I have written an article on electrical oscillations, and others. I have collaborated with Dr. Webster in his book on Mechanics and with Professor Noyes on a book that he has written.

Q. Were you present during the tests conducted by Dr. Chaffee at the Cruft High Tension Laboratory, Harvard University on July 3rd and 4th of a Simpson Mercury Valve Transmitter, and in which a Braun tube was employed to show oscillations?

This question and all others of like import and also any and all exhibits relating to the same are objected to as incompetent and immaterial for reasons heretofore stated in respect of all the plaintiff's witnesses at this 9266
session, i. e., with the apparatus referred to under the name of Simpson Mercury Valve Transmitter and purporting by the plaintiff to be the defendant's apparatus in toto is not in fact defendant's apparatus, so that any testimony of these scientific gentlemen concerning the same cannot possibly have any bearing on the issues in this case. A. I was.

Q. Did you observe the method employed by Dr. Chaffee in conducting his tests? A. I came to the Cruft Laboratory with the intent of seeing whether these experiments were conducted according to proper methods and to be sure that they indicated properly the phenomena which they were supposed to indicate. I have criti- 9267
cally examined the connections of the apparatus to the Braun tube, the technique employed in the use of the Braun tube, and have been unable to find any fault with the way in which these experiments were conducted. I have no personal interest in wireless apparatus of any description except that of a scientific nature, and therefore feel that I can judge entirely impartially the results of the experiments.

Q. In your judgment were the methods used by Dr. Chaffee fair, accurate and proper methods? A. As I

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have just stated, I have been unable to find any fault whatsoever with the experimental arrangements used by Dr. Chaffee in his Braun tube apparatus as connected with the Simpson Mercury Valve Transmitter.

Q. During the test was or was not the transmitter connected and assembled as shown in defendant's exhibit Simpson Drawing F. G. S. 2 and defendant's exhibit F. G. S. 7? (See Vol. 2, pp. 1080, 1131.) A. I examined the connections of the Simpson Mercury Valve Transmitter in Dr. Chaffee's laboratory and they agreed with Diagram marked F. G. S. 2 except in two particulars. One is that
9269 there was a circuit called a keep-alive circuit that is not shown in this diagram, and another is that instead of a real antenna a dummy antenna consisting of a condenser, a resistance and an inductance was used to replace it. This dummy antenna included an ammeter. I notice in F. G. S. 7 that these two circuits have been inserted, and my inspection showed that the apparatus was connected, as represented diagrammatically in this last figure.

Q. In your judgment was the transmitter arranged and connected as you have described above in proper adjustment for normal operation during the tests which you witnessed? A. I consider that a quenched spark wireless transmitter is operating normally when the average
9270 antenna currents square is at a maximum and when the apparatus is connected up as indicated by a diagram furnished by the maker and when the spark gap is sparking properly. These conditions were fulfilled most of the time during which the tests were made. As nearly as I can remember, the photographs were taken when these conditions were best fulfilled.

Q. Based on your observations made during these tests, what is your conclusion as to the oscillatory character of the primary circuit current in the Simpson Mercury Valve Transmitter? A. You mean when normally connected?

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Q. Yes. A. My conclusions are based upon two kinds of evidence, what I saw with my eye and what I saw in photographic plates. From my knowledge of experimental physics, I wish to state here that to show visually by any means whatever oscillations of the frequency shown in these experiments is a remarkable scientific achievement. I would like to explain about the oscillograms that I saw. I have here a paper published in the Proceedings of the American Academy of Arts and Sciences, in which are shown photographs of oscillograms obtained by Dr. Chaffee. If the plates at the end
of this article are examined, there will be found photo- 9272
graphs of oscillations occurring in electrical circuits and the point which I wish to bring out is that in that part of the oscillation where the spot of light moved by the magnetic effect of the current (or any other effect that will produce a motion of the spot of light) is most rapid, the trace of the spot in the photographic plate is very dim. In an oscillation the spot of light momentarily stops at the upper and lower ends of its path and therefore produces a greater effect both on the eye and the photographic plate at those points. I refer in this description to diagrams b, c, e, plate 7. On plate 5, diagram b, is shown the oscillogram of a number of oscillations de-
creasing in amplitude, and I want note to be taken that 9273
the bright part of this picture is produced by the series of points which lie at the ends of the oscillations, producing the effect of an arrow. I have made a slight computation which may be of interest in interpreting these photographs, which is that the spot of light moves over the phosphorescent screen with an average speed of from five to over ten miles a second, and I consider it a re markable achievement to have been able to give an impression both visual and photographic of such a rapid motion. With these remarks in mind it is not a priori to

9274 be expected that extremely sharp and clear pictures are to be produced. Another point is, that the photographs are not due to one single train of waves, but are due to the combined effect of several hundred of them, the exact number of which I am not sure, and in order that they may accentuate the image they must lie exactly on top of each other for a considerable length of time. This requires an extreme regularity in the working of the transmitter. This extreme regularity was obtainable at times, but as its continuance could not be predicted, very often a slight change would take place while the photographic
9275 plate was being exposed, and hence a blurring in the picture. As a man used to examining scientific observations and judging of their validity, I consider that from both my visual observance and the photographic corroboration of my visual observance that undoubtely there are oscillations in the primary circuit of the apparatus under investigation.

Q. In normal working how many oscillations did you observe? A. Many times when the apparatus was working normally I have observed oscillations as shown by the oscillograms in the Braun tube. There were two methods of determining that such oscillations existed, one being to allow the spot of light to be deflected by a
9276 microscopic portion of the current in the primary circuit, and it was found that the spot was drawn out on both sides of its zero position. The drawn out portion contained bright spots on the screen (dark spots on the photographic plate) which showed the figure to be expected if such oscillations were present. In order to make it more evident that these spots were nothing else than the tops of the oscillations, the figure was drawn out by means of an electrostatic deviation (produced in the spot in a perfectly legitimate manner) and many times also have I seen the actual so-called wave train

similar to figure f plate 6 of the Chaffee article above referred to. At my suggestion, in order to make this more conclusive, two aspects of the spot were photographed on the same plate, first, when the spot was drawn out by the oscillatory current in the primary circuit showing what I believe have been called beads on the drawn out portion. This figure was then drawn out at right angles itself, and I saw a wave train which corresponded exactly with the figure to be expected if the linear figure was produced by oscillations.

Q. Defendants made *ex parte* tests of their apparatus
at Washington University, Seattle, Washington, in 9278
which tests it appears a Braun tube was employed, having outside deflecting plates connected across the spark gap of the transmitter. Based on your knowledge as a scientist and your observation of the tests made by Dr. Chaffee, what weight, if any, can be given to conclusions based on those Washington University tests? A. There are two vital objections to the Washington University tests. One of them is that when outside plates are used, no reliance can be placed upon the observed deflections of the spot. Secondly, assuming for the moment, that the plates as thus used were to produce deflections in
the spot of light proportional to the potentials which 9279
existed around the spark gap, the oscillogram thus produced would not show either the presence nor the absence of oscillations in the primary circuit. I make the last statement because when the gap is on the point of discharging there is a large electromotive force around it, which would produce a large deflection in the spot of light. At the discharge of the gap the potential around the gap would become very much smaller than it was before, I should say roughly 100 times smaller. The action of the spot in this case would be, still assuming the plates to act correctly, that the spot would be drawn

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out a certain distance, approximately, as shown by reproduction of Simpson photograph, Exhibit FGS 4, an inch. On the discharge of the gap the spot would return to its zero, and if there were oscillations they would be hidden in the broad bright spot on this plate FGS 4. This description of what would take place does not purport to explain any of the photographs on this plate.

Defendant gives notice, pursuant to the stipulation and order as before, that he will examine Melville Eastham, Cambridge, Mass., as a witness on
9281 its behalf at the office of Philip Farnsworth, Esq., 149 Broadway, New York City, on Tuesday, July 11th, at 10 o'clock, A. M.

Adjourned to 10 A. M. Saturday.

Cambridge, Mass., July 8, 1916.

Met pursuant to adjournment.
Present: Counsel as before.

9282 DIRECT EXAMINATION OF THE WITNESS COFFIN RESUMED.

(Statements of Counsel.)

By Mr. Peters: In view of the objections made by defendant's counsel to the Chaffee tests based on the use of the 500 cycle Marconi generator used to supply alternating current, I desire to make the following statment and offer:— I am informed that these defendants employed a 500 cycle generator in making their Washington University tests

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and that they repeatedly stated to the plaintiff that any commercial generator of any commercial frequency might be employed with the set, and we have therefore employed the 500 cycle commercial generator. As the use of it has now been objected to, plaintiff offers the generator to the defendant for a thorough test and examination.

Furthermore, the plaintiff offers to ship the generator to Seattle by express in order that it may be examined and tested by the defendant in Seattle and by the assessors. As considerable expense will be involved in carrying out this offer, 9284
I now ask the defendant's attorney if he accepts the same, it being understood that the tests will be made in the presence of the assessors and plaintiff's representatives.

By Mr. Farnsworth: Defendant most certainly does accept the plaintiff's offer to ship that Marconi generator to Seattle for test, but it is further requested that there be sent not only said generator, but also the other things used therewith in the so-called Chaffee tests, viz:—the three rheostats, including (1) the starting box, (2) the field rheostat and (3) the speed control rheostat; also the reactance used in series with the primary of the 9285
transformer and the dummy antenna and all apparatus used connected therein and therewith, and the protective device used to protect the motor generator from surges. As to the dummy antenna and apparatus used therewith, the same should include the indicating instrument with its shunt used to indicate the current in the dummy antenna during the Chaffee tests, also all the rest of the apparatus connected in and with said dummy antenna. We would also like the stroboscope used

in the tests in which the Marconi generator was used.

By Mr. Peters: A portion of the apparatus called for, as defendant's counsel has been informed, is the property of the Cruft Laboratory, namely, the stroboscope, the non-inductive resistance used in the dummy antenna and the indicating instrument. These things are just as accessible to and just as much under defendant's control as they are accessible and under the control of the plaintiff. The plaintiff cannot of course undertake
9287 to send these things to Seattle. Defendant, however, has every opportunity to inspect and examine them at the laboratory. The other apparatus will be sent by express to Seattle as soon as it can be packed and shipped, including the Simpson transmitter.

By Mr. Farnsworth: Defendant suggests that the shipment be made by express in order to be in time.

By Mr. Peters: I have several times stated that the shipment will be by express.

By Mr. Farnsworth: Of course the burden is exclusively upon the plaintiff with respect of ship-
9288 ping for tests in Seattle all the apparatus used in tests here in Massachusetts. The defendant would share responsibility only in case the plaintiff had accepted defendant's offer for *inter partes* tests. The plaintiff not only declined defendant's said offer of *inter partes* tests but elected to have its tests made far from Seattle, after the time when the assessors had been appoined.

Defendant, in the interest of truth and justice, would be willing and would most gladly welcome complete tests of all kinds in the presence of the assessors in Seattle, such tests to be made *inter partes* and in the presence of the assessors and of the court, provided only that it was defendant's apparatus which was tested, and not otherwise.

Defendant therefore here and now renews its offer for *inter partes* tests, which the plaintiff has heretofore refused, and offers to plaintiff another opportunity to accept inter partes tests, this time to be conducted in the presence of the assessors and the court in Seattle at the resumption of the trial this month.

By Mr. Peters: The tests just concluded were 9290
made under an order of court which provided, at the defendant's request, that they should not be considered *inter partes* tests. That provision, as I am informed, was put in the Order at the express request of the defendant. The tests were made in the East because, as I am informed, the Cruft Laboratory is the only place where tests of this kind can properly be made.

In shipping the apparatus back to Seattle, plaintiff does not undertake to repeat the tests in Seattle, but is offering the apparatus to defendant to examine and test as it may see fit. They can
9291
make further tests here, if they desire, or they can take the apparatus when it arrives in Seattle and make the tests. If suitable apparatus can now be found in Seattle to test this apparatus, which was not the case at the time of the trial, plaintiff will be only too glad to have the tests repeated by properly skilled parties.

By Mr. Farnsworth: The plaintiff, in reply to defendants proofs, is endeavoring to prove something. Defendant has always offered all of its apparatus to plaintiff for tests, freely and fully.

9292 Joseph George Coffin—Direct.

Through plaintiff's omission, possibly inadvertent, the plaintiff has not obtained defendant's apparatus for tests as yet, or, having had defendant's apparatus, has not seen fit to test it. The record will show on which party is the fault of not conducting here in Cambridge tests of defendant's apparatus.

All that defendant has to say now in respect to these attempts of plaintiff to prove something is that defendant continuously extends its offer for *inter partes* tests in the presence of the assessors
9293 and the Court of apparatus which is actually defendant's appartus. Defendant has made its proofs and plaintiff is endeavoring to reply to them. Let the plaintiff reply properly, if it can.

Q. You have stated that you observed during the Chaffee tests that when the Simpson transmitter was operating normally oscillations occurred in the primary circuit. What can you say as to the number of oscillations? A. In the tests there were two opportunities provided for observing the number of oscillations existing in the primary circuit of the Simpson Valve transmitter when operating normally; the first being the so-called
9294 straight line magnetic deflections. In these, I have observed many times oscillations as indicated by the bright spots on these diagrams amounting to 2, 2½ and sometimes 3 complete oscillations, at least. The second opportunity being those experiments where the straight line deflection was drawn out so as to show the actual wave train. I have seen in these, whenever the apparatus was working normally, 2, 2½ and sometimes 3 complete oscillations.

My visual observations are corroborated by photographs taken at the time that I was visually observing

3099

iff's Exhibit No. 67c.

scillogram of the current
in Primary Circuit.

am #1

Oscillogram of the
Current in the
Secondary Circuit
or antenna.

 9301

these oscillograms, and they show, both on the straight line magnetic deflection of the spot and on the drawn out wave train, 2 and 2½ complete oscillations. The photographs being the sum of many wave trains, and the exposure lasting from three to ten seconds, show the result of many super-imposed motions of the spot. The eye being able to form a picture almost instantaneously of what is going on, is therefore able to see more clearly, and I attribute the possiblity of seeing sometimes more oscillations visually than photographically to this fact.

Q. Will you please make a diagram showing the
oscillations as you observed them during the normal 9302
operation of the Simpson transmitter, both in the primary and in the antenna circuit. A. You mean reproductions of what I actually saw?

Q. Yes. A. I have drawn in pencil a reproduction, slightly enlarged, as nearly as I can remember, of what I saw on the screen many times.

In my diagram 1 (reproduced opposite), the first sketch shows an oscillogram of the current in the primary circuit of the Simpson valve transmitter. The beads or spots marked a a′ I consider to be the successive maximum and minimum of the
first oscillation above and below the zero position 9303
of the spot d, the points b and b′, the maximum and minimum of the second oscillation, the points c and c′, those of the third. The point c′ was not always evident, but I have seen it several times. Immediately after observing this first oscillogram I asked to have the up-and-down motion drawn out horizontally, and if my interpretation of this first diagram is correct it should give the picture of a highly damped wave train. What I saw when the picture was drawn out I have indicated in the second sketch of my diagram #1, p. 3100.

The points marked a a′ in the second sketch cor-

Plaintiff's Exhibit No. 67c.

Oscillogram of the current in Primary Circuit.

Coffin Diagram #1

Oscillogram of the Current in the Secondary Circuit or antenna.

Coffin Diagram #2

District Court of the U.S.
Western District of Washington
Northern Division.
Marconi Wireless Telegraph Co. of Am.
v.
Kilbourne & Clark Mfg. Co.
Plaintiffs Exhibit
"Coffin Diagrams Nos. 1 and 2"
G. C. A.
July 8. 1916. R. P.

9304

responded exactly to the points a a′ in the first sketch; the points b and b′ in the second sketch corresponded to those marked b and b′ in the first sketch; the point c in the second sketch corresponds to the point c in the first sketch, and several times I have seen the point c′ in the second sketch which coresponded to the point c′ in the first sketch, which I have sometimes seen.

In my diagram #2 (see p. 3100) I have reproduced as nearly as I can remember on a slightly enlarged scale, what I saw when the spot was magnetically deflected by the current in the antenna circuit and the deflections
9305 drawn out into a wave train.

By Mr. Peters: I offer in evidence the sheet of drawings produced by the witness. The same is marked "Plaintiff's Exhibit, Coffin diagrams 1 and 2." (Plaintiff's Ex. No. 67C.)

Q. You were present and heard Dr. Chaffee's testimony regarding the tests resulting in the photographs which he has produced. Do you agree with the conclusions expressed by Dr. Chaffee in such testimony?

Mr. Farnsworth: Objected to as vague and indefinite.

9306

A. I was present most of the time during Dr. Chaffee's testimony, and I have read Dr. Chaffee's depositions, and I cannot help but agree with his conclusions.

Q. Please state briefly the circumstances under which you attended Dr. Chaffee's tests. A. One day during the week preceding the one beginning July 1, the exact date of which I do not remember, but which I have in my note book at home, Mr. Waterman telephoned me at my house, asking me if I would be able to go to the Cruft High Tension Laboratory, to witness some tests which were being conducted by Dr. Chaffee, and testify whether

 9307

or not these experiments were conducted according to correct scientific methods, and to observe and testify as to what the results of these experiments would show.

(Direct examination closed.)

CROSS EXAMINATION.

XQ. (By Mr. Farnsworth) Please enumerate the papers you have read or published which relate expressly to the subject of radio telegraphy, giving the titles of the papers, and the place of reading, or publication, or 9308
both. A. In the Bureau of Standards, I have published two articles, one entitled "On the construction and calculation of standards of self inductance", another "On the influence of frequency upon the self inductance of coils."

This latter paper was published in a slightly different form in the proceedings of the American Academy of Science.

I have also published a paper "On the influence of frequency upon the capacity of absolute condensers", in the Physical Review.

Another paper in the same place entitled "Influence of frequency upon inductance of coils of N-layers." I 9309
have written a short article in the proceedings of the Institute of Radio Engineers "On the calculation of high frequency oscillations in circuits of any kind", which includes, of course, those circuits which appertain to wireless telegraphy.

I have also written a paper entitled "Expansion of Maxwell's equation for the mutual inductance between two circles." (I am not sure about the exact title of this last paper. This was published in the Physical Review.)

XQ. Each and all of those publications you have enumerated are entitled and relate expressly to radio telegraphy, in terms, as I understand you? A. I don't understand your question.

XQ. Will you please, in respect to the papers you have enumerated, give the exact data of the publications in which they may be found? A. This information, of course, can be furnished after a slight search, but I am unable to answer that question at the present time.

XQ. And you are unable to state whether each and all of these papers you have referred to relate expressly to the subject of radio telegraphy in terms? A. The sub-
9311 ject matter of these papers certainly does relate to the subject of wireless telegraphy in one part or another, in proof of which my work is cited in Dr. Zenneck's book.

XQ. Any of them have the title "Radio Telegraphy", or anything like it? A. No, sir.

XQ. Do the articles themselves, any of them, refer to radio telegraphy, as you remember? A. Not expressly so stated.

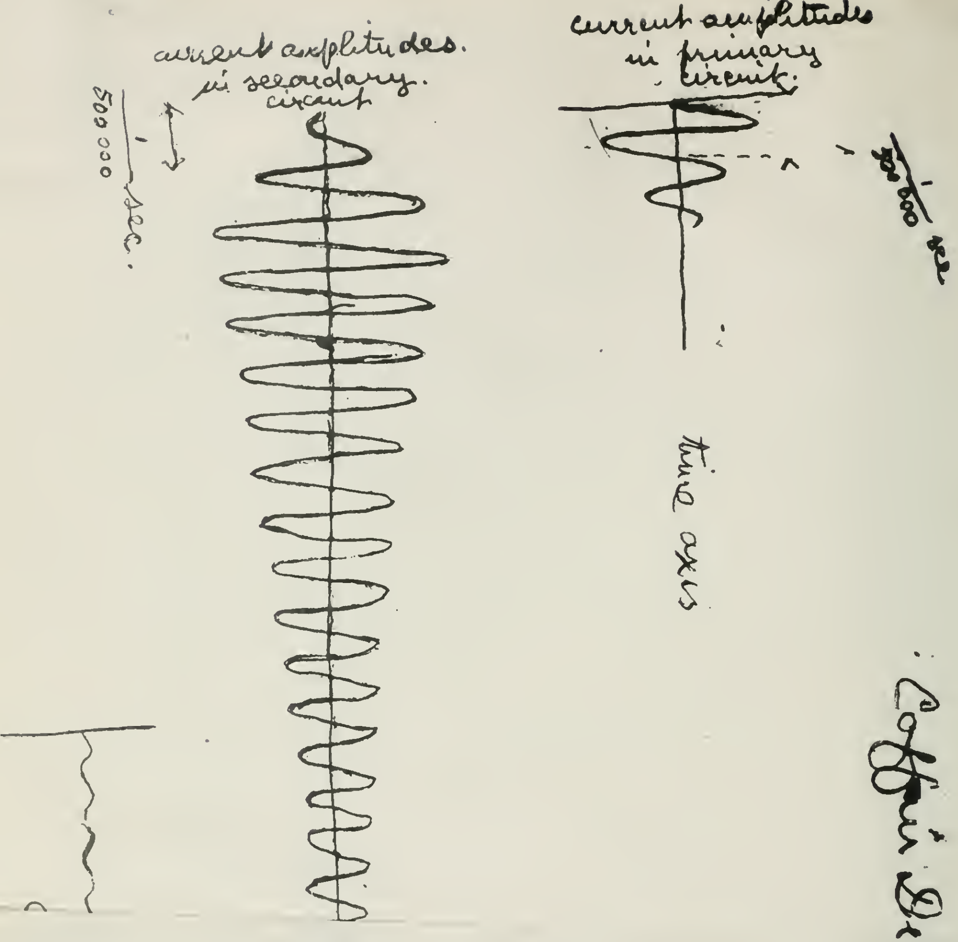
XQ. Please calculate the number of oscillations in the dummy antenna of Dr. Chaffee's tests of the Simpson mercury valve transmitter. A. Assuming in this antenna the capacity to be 1/1000th of a micro-farad, and the inductance to be around 100 micro-henrys and
9312 the resistance to be from 6 to 8 ohms, I calculated the decrement of this circuit to be from .06 to .08. The number of oscillations corresponding to those limits is from 69 to 57.

XQ. Please calculate those antenna calculations as closely as you can, on the basis of a decrement of .08. A. What do you mean by calculate?

XQ. I mean calculate. A. You say the numerical——

XQ. The number of oscillations. A. I have given them.

XQ. You have given them already on the basis of a


current amplitudes in primary circuit
current amplitudes in secondary circuit
time axis
1/500 000 sec.

 9319

decrement of .08? A. Oh, on a basis of .08, the number of oscillations in the antenna circuit comes out to be about 57, assuming the wave length to be 600 meters.

XQ. In respect to those tests of the Simpson mercury valve transmitter and of the supposed spark circuit activity, and of the number of oscillations in the dummy antenna; that is, 57, please make a diagram illustrating the conditions in those two circuits. A. I have drawn in "Coffin diagram No. 3" (reproduced opposite), two curves, the first showing the current curve in the primary circuit; the second showing the corresponding current curve in the secondary circuit. It is to be 9320
understood that the number 57 given for the number of oscillations in the antenna circuit is based upon the accepted method of enumerating these; that is, the number of complete oscillations taking place before the current amplitude reaches 1 per cent of its maximum amplitude.

By Mr. Farnsworth: "Coffin diagram No. 3" is offered in evidence as defendant's exhibit.

XQ. This occasion of the test of the Simpson mercury valve transmitter is the first occasion in which you have been employed by the Marconi Company in respect to the Marconi patent in suit? A. No. 9321

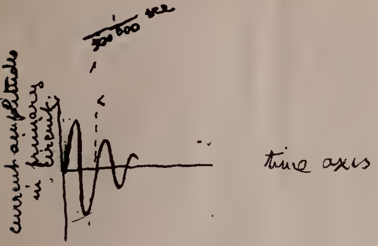
XQ. What other occasion? A. I have made an affidavit in reference to this patent on a previous occasion, the date of which I do not remember.

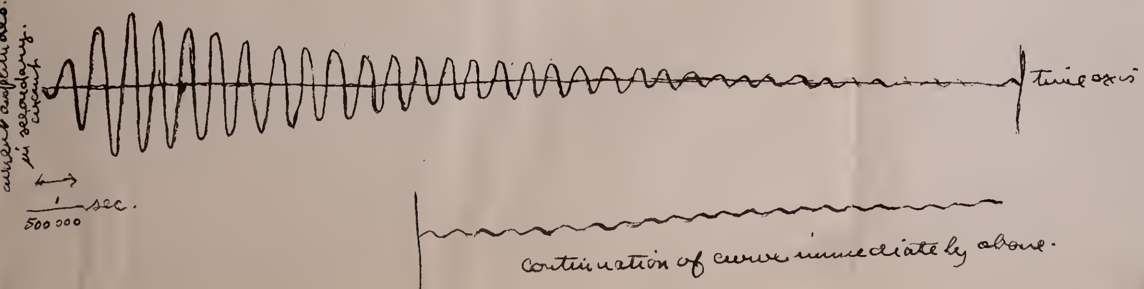
XQ. What was that occasion? A. I was called in to read over Dr. Kolster's affidavit, and express my opinion as to what his affidavit showed.

XQ. That is, in connection with the Simpson mercury valve transmitter? A. I understand it to be so.

XQ. What occasion was that, do you remember? A. I think it was this same case.

Coffin's Diagram #3





P3106

9322

XQ. You made an affidavit in the case you refer to? A. Yes.

XQ. I show you a copy of an affidavit, sworn to by Joseph George Coffin on January 31, 1916, the affidavit being headed as follows: "United States District Court, Western District of New York. Marconi Wireless Telegraph Company of America, plaintiff, *v.* Detroit and Cleveland Navigation Company, defendant. In equity on Marconi patent No. 763772." Is that the affidavit to which you have just referred? A. It is.

XQ. Of yours? A. It is.

9323 XQ. In a case against the Simpson Mercury Valve Transmitter? A. If Detroit and Cleveland Navigation Company means that, yes.

XQ. That is your affidavit, which I have just shown you? A. I believe it is.

> By Mr. Farnsworth: I offer the affidavit in evidence for defendant; defendant's Exhibit "Coffin affidavit, Buffalo suit".

XQ. That Buffalo case was the first occasion, was it, in which you performed any service for the plaintiff Marconi Company in connection with the Marconi
9324 patent in suit here? A. It is not.

XQ. What then? A. I was called upon by the Marconi Company to make calculations for them in regard to certain circuits which they presented to me for solution, in a case whose exact title I do not know.

XQ. That was a case, wasn't it, under the Marconi patent here in suit, a little over a year ago, before Judge Veeder, in Brooklyn, against the Atlantic Communication Company? A. I believe it is.

XQ. And in said case, you were retained to be prepared as a witness for the Marconi Company in that

 9325

case, although you in fact did not finally testify? A. I do not know.

XQ. Do not know what? A. Whether I was to be retained for a witness or not. I was employed to make calculations for them.

(CROSS-EXAMINATION CLOSED.)
(DEPOSITION CLOSED.)
(Signed) JOSEPH G. COFFIN.

9326

(STATEMENTS OF COUNSEL.)

By Mr. Peters: I am informed by Mr. Sykes of the Cruft Laboratory that the indicating instrument which defendant has requested shipped to Seattle, but which is the property of the Laboratory is now available and can be inspected, tested and calibrated by defendant, if defendant desires. I note that Mr. Simpson is present.

By Mr. Farnsworth: We make no particular request. What defendant does is to make its continuing offer to take part in *inter partes* tests of the true Simpson mercury valve transmitter set, 9327 preferably in the presence of the assessors of the court in this case.

In view of plaintiff's refusal of our previous offer of *inter partes* tests, it should b eclearly understood we accept no responsibility whatsoever in connection with the present *ex parte* tests, which we were served with notice that we might attend, and for which, of course, we had no opportunity of preparation, providing the proper apparatus, for checking up in any manner whatsoever the results obtained in these *ex parte* tests. Whatever the defendant may choose to do here, or may choose to have done is entirely a separate question from the responsibility with which the plaintiff voluntarily elected to assume of conducting *ex parte* tests three thousand miles from the court, and from defendant's residence.

By Mr. Peters: Defendant's representative, Mr. Simpson, has stated that he would like to check the accuracy and calibration of the instrument referred to, which is the property of the
9329 Cruft Laboratory, and he is now invited to test, check, and calibrate the same.

By Mr. Farnsworth: Defendant wishes to make any proper reply to plaintiff's proofs. We again serve notice that the proper manner to do that is by *inter partes* tests, in Seattle, in the presence of the court and of the assessors. We again repeatedly refuse to accept any obligation as the result of defendant's fault in conducting *ex parte* tests here, at the end of plaintiff's time to take proofs.

By Mr. Peters: Please say whether or not you accept my offer.

By Mr. Farnsworth: Mr. Simpson may do
9330 whatever he chooses to do.

By Mr. Peters: Inasmuch as I am arranging at considerable expense to ship the generator employed in the Chaffee tests to Seattle, may I inquire again whether defendant requests that this be done, and desires to test the same in Seattle?

By Mr. Farnsworth: I repeat what I have already said *ad nauseam.* Defendant now, and in the future, as always in the past, is more than willing and desirous of co-operating in *inter partes* tests of the Simpson mercury valve transmitter,

 9331

preferably in the presence of the Court and of the assessors. Anything further than this is exclusively up to the plaintiff to arrange for, as a part of its attempted reply to defendant's proof. Further than that, it is not necessary to say anything. The plaintiff may do as it chooses in the attempt to reply to defendant's proof. In respect to the Simpson mercury valve transmitter, the plaintiff at its peril may refuse or neglect to do anything in respect of the apparatus here which it may deem to be proper to be done before the
Court and assessors in Seattle, by way of conduct- 9332
ing some real tests, with some actual apparatus of defendant.

(Offer of Exhibits.)

By Mr. Peters: I offer in veidence, for the purposes of identification, the following apparatus used by Dr. Chaffee in connection with his tests.

Simpson mercury valve transmitter, mounted on panel.

By Mr. Farnsworth: What do you mean by
"Simpson mercury valve transmitter"? 9333

By Mr. Peters: I mean the apparatus supplied by the defendant, as being of its manufacture.

By Mr. Farnsworth: Including the generator?

By Mr. Peters: It does not include the generator.

By Mr. Farnsworth: Where is the Simpson, Kilbourne and Clark generator, which the de-

9334

fendant Kilbourne & Clark Manufacturing Company furnished the Marconi Company for the purposes of tests?

By Mr. Peters: It will be produced in due course.

By Mr. Farnsworth: Very well.

By Mr. Peters: Motor generator used in tests at Cambridge, Massachusetts.

Starting rheostat for motor generator used in tests at Cambridge, Massachusetts.

Protective device used in tests at Cambridge,
9335 Massachusetts.

Mercury valve belonging to Simpson transmitter, used in tests at Cambridge, Massachusetts.

Condensers (two jars and rack) used in dummy antenna in tests at Cambridge, Massachusetts.

Reactance used in power circuit, primary, in tests at Cambridge, Massachusetts.

Transformer of Simpson mercury valve transmitter used in tests at Cambridge, Massachusetts.

Field rheostat of generator used in tests at
9336 Cambridge, Massachusetts.

Motor field rheostat used in tests at Cambridge, Massachusetts.

Inductance, three turns of which were used in dummy antenna in tests at Cambridge, Massachusetts.

Whereupon CHARLES R. CROSS, a witness called on behalf of the plaintiff, being first duly sworn, testified as follows:—

DIRECT EXAMINATION BY MR. PETERS.

Q. State your name, age, residence and occupation. A. Charles R. Cross; my home is Brookline, Mass.; age 68 years; occupation teacher.

Q. What experience and training have you had which qualified you in the matter of tests relating to electrical apparatus, particularly the measurement and detection 9338
of high frequency oscillations by the Braun tube method? A. I am the Thayer Professor of Physics at the Massachusetts Institute of Technology and Director of the Physical Laboratory of that institution, having occupied these positions for over 30 years.

I have had the general charge and oversight of the work of the Department of Physics in the Institute ever since 1877, and during this whole time have given many lectures embracing the subject of electricity in its different branches, that is, in the experimental but not the mathematical portions of this subject. During this period I have had occasion to purchase and use almost all the principal forms of electrical apparatus, includ- 9339
ing those employed in the study of high frequency phenomena and electrical oscillations. When the X-Rays were first discovered I made, in company with some of my students and colleagues, a very considerable number of experiments, using the Crookes vacuum tubes which I had purchased perhaps ten years before, producing by means of them radiographs prior to the construction and introduction of X-Ray tubes. Indeed, in the first surgical operation performed in New England in which the X-Rays were used in diagnosis the radiographs were taken by myself and students in our laboratory.

I have given many special courses of lectures to my students upon the phenomena of electrical oscillations and waves.

I have given before the Lowell Institute in Boston two courses of lectures devoted chiefly to the phenomena which obtain in vacuum tubes. I have had constructed under my supervision various such tubes for particular studies, and one set of this character has proved of sufficient interest for teaching purposes to have been introduced into the stock apparatus of several German manufacturers. In 1893 I was chairman of one of the
9341 three electrical sections at the World's Columbian Exhibition at Chicago, and I was appointed to a position as chairman of the section relating to radio activity, and if I recollect rightly, X-Rays at the St. Louis Exhibition early in 1900, although I was unable to attend this last.

I have written a number of papers relating to electrical subjects, but none relating to wireless telegraphy. These papers are chiefly published in the proceedings of the American Academy of Arts and Sciences.

I am Chairman of the Rumford Committee of that Academy, and have been such, I think, for nearly 20 years, which committee has charge of one of the most important American funds employed in furtherance
9342 of research in physics.

This is rather a long story, but then I have taught for a great many years.

Perhaps I ought to say in this connection that in 1882 there was established at my suggestion and instance a course in the Institute leading to a degree in electrical engineering, the first to be established in this country, and one of the first three or four in the world. I was at the head of this course as well as that of Physics for 20 years, but during the last ten years have been able to de-

 9343

vote myself wholly to Physics, which has always been my specialty rather than engineering.

Q. Were you present at the tests of the Simpson transmitter at the Cruft Laboratory, Harvard University, conducted by Dr. Chaffee July 3rd and 4th? A. I was.

Q. Did you observe the method used by Dr. Chaffee in conducting his tests? A. I did, with great care.

Q. In your judgment were the methods used by Dr. Chaffee fair, accurate and proper methods? A. In my opinion they were, beyond the possibility of doubt. The results which he reached were such as required appara- 9344
tus and methods of the utmost delicacy to secure and altogether apart from the particualr objects of the tests in this case I think that as a whole they constituted a most beautiful scientific research, which would be worthy of publication in any physical journal in the world.

Q. Did you make observations during the tests to ascertain the character of current flowing in the primary closed circuit forming a part of the transmitter? If so, please state your observations and the results. A. Referring to the diagram which shows the arrangement of the apparatus and circuits (Chaffee Diagram 1), I observed in the effect produced upon the fluorescent screen
in the Braun tube when the magnetizing coils with which 9345
that tube was furnished were caused to be traversed by the current which flowed through the primary of the apparatus on the production of the spark discharge. Prior to the excitation of the primary circuit, the fluorescent screen showed a small, round illuminated spot due to the striking against it of the cathode stream produced by the high voltage battery employed for that purpose.

It is a well-known fact upon which the employment of the Braun tube is based, that in a magnetic field such as is produced by a current traversing the external magnetizing coils the cathode stream is deflected in proportion to the strength of this magnetic field. Hence, if the magnetic field varies in magnitude or direction, the cathode stream will move in correspondence with these changes and the fluorescent spot which marks the place of incidence of the cathode stream with the fluorescent screen will therefore move in like manner. Therefore, the passage of a current varying in magnitude and direction through the magnetizing coils will be indicated by the motions of the spot.

There is no device whereby such current changes may
9347 be indicated which is comparable in sensitiveness to that which is employed in the Braun tube, as I am explaining, since the corpuscles composing this stream are of almost inconceivably small magnitude, having a mass only about 1/1800 of the mass of the molecule of hydrogen.

But, sensitive as is this cathode stream, yet when it is to be employed, as Mr. Chaffee has done, for the detection of the excessively rapid oscillations present in wireless telegraphy apparatus, oscillations lasting only about 1/500000th part of a second, a serious problem presents itself. While with a slow oscillation, lasting 1/100th or perhaps 1/1000th of a second the trace
9348 of light due to a single vibration of the cathode stream might be observed, yet when it comes to the excessively rapid vibrations which Mr. Chaffee set himself to study, the motion is entirely too rapid to be observed by the eye. It is therefore necessary to use some sort of a circuit-breaking apparatus which shall produce a series of discharges, many in succession, but of such similarity of character and produced at such precisely equal intervals of time that each successive discharge and corresponding current through the magnetizing coils shall produce motions of the cathode stream and spot that are substantially identical with

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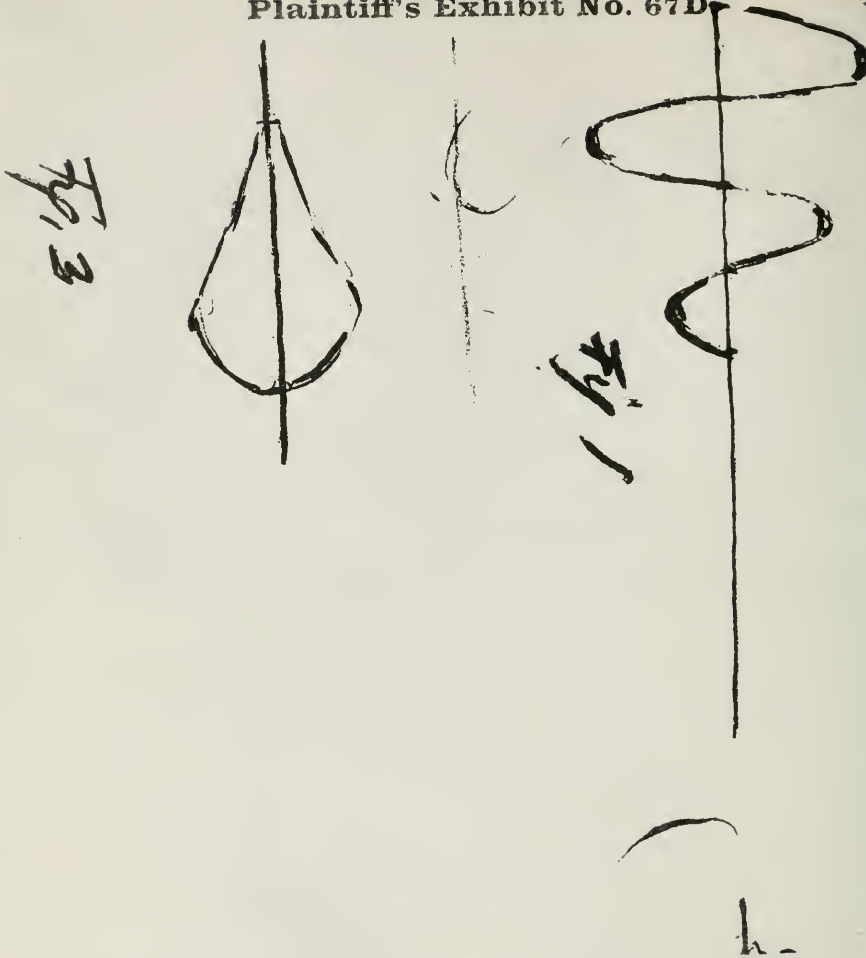
Plaintiff's Exhibit No. 67D



one another, so that to the eye the appearance shall be substantially the same as if it could see the single oscillation.

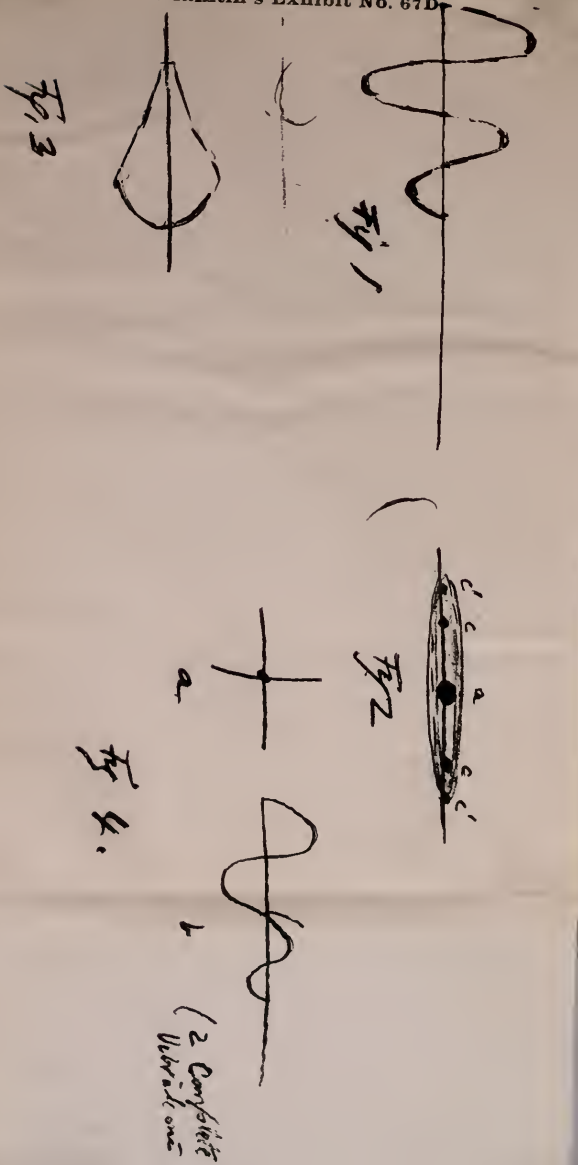
All this Mr. Chaffee has succeeded in doing with remarkable perfection.

I have thought it best to make this preliminary statement although I am aware that in the depositions previously taken at this hearing all these facts have been clearly set forth, thinking that my own reply might be thus made clearer.

I have already stated that when the primary circuit
was devoid of current the cathode spot, as I will call it, 9356
was at rest at or near the centre of the diaphragm. When currents were sent through the primary circuit of the apparatus in connection with which the Braun tube was placed, phenomena were observed which I can best make clear by referring to some rough diagrams which I have drawn to illustrate the facts (reproduced opposite as Pltff's. Ex. 67D).

Referring to Fig. 2, on the sheet which I produce, the position of the cathode spot when undeflected is that indicated at a. On the passage of current through the primary circuit there is observed immediately and always
a spreading-out of light through a certain length and 9357
about equally on either side of the original position of the spot, although there are minor quantitative differences as to extent. When current is traversing the primary circuit and the elongated band of light to which I have just referred is produced, the cathode spot disappears, since it is owing to the motion of this spot to and fro that the luminous band is due. The presence of such a luminous band on either side of the undisturbed position of the cathode spot would lead one to suppose, and correctly, that there was at least one complete vibration of the spot.

Plaintiff's Exhibit No. 67D



9358 This, however, is by no means the most significant phenomenon which is observed. On either side of the initial position of the cathode spot are seen brighter spots, of which under favorable circumstances there are four. These spots are shown at c, c, c′, c′ in Fig. 2. Those marked c are equi-distant from what I will call the zero position of the cathode spot; those marked c′ are also equi-distant from one another, but further from the zero point.

These characteristic appearances indicate beyond the possibility of doubt that the cathode stream has been
9359 set into vibration by the current traversing the magnetizing coils. For the brighter spots which I have described are evidently due to the reversal of direction of motion of the cathode stream at the end of each half vibration, since with the reversal of the direction of motion at the end of the path the movement is at its slowest, and hence impresses the eye most strongly, since the brightness of an evanescent illumination is proportional to the duration of the impression.

I do not see how these demonstrations, even if carried no further, could fail to be absolutely convincing as to the presence in the primary circuit of the apparatus under consideration of at least two complete vibra-
9360 tions. Probably there are more, but it is difficult to observe any nearer the middle point.

By an extension of these tests, however, Mr. Chaffee has succeeded, so to speak, in making assurance doubly sure.

It is a usual method in the study of low-frequency longitudinal oscillations to view these in a rapidly-rotating mirror whose axis is parallel to the line of displacement of the vibrating point. Under these circumstances a sinuous line will be produced. Thus, a vibration like that indicated at a, Fig. 4, would be drawn into a sinuous line like that represented at b in the same figure.

This comparatively crude method, however, could not readily be applied and I imagine could not be applied at all to the study of such excessively rapid vibrations as Mr. Chaffee had to deal with. He was, therefore, under the necessity of devising some new method wherewith he could combat his particular problem with some hope of success.

It will not be necessary for me to describe this method in detail as it has already been considered with clear-
ness. It will suffice to say that by combining the electro- 9362
static deflection produced by two interior plates in the Braun tube with the electromagnetic deflections which I have been considering he was able to produce a time axis to which the vibrations of the cathode stream can be referred. This was done by causing a very slow discharge from a condenser to take place, thereby varying the potential of the electrostatic plates with which it was connected. It was arranged so that this took place simultaneously with the passage of the current and during the duration of a single test, these potential plates caused an approximately uniform displacement of the cathode stream in a direction at right angles to the vibra-
tions produced by the electromagnetic coils. 9363

When thus drawn out along the time axis, the curve takes the appearance indicated in Fig. 1 of my drawing; that is, the heavily shaded portions of the curve at the extreme of each half oscillation are seen brighter than surrounding portions, since it is at this point that the motion is slowest.

I find, therefore, in this modified manner of studying the vibrations of the cathode spot, most abundant confirmation of the correctness of the inference which I drew from the study of the rectilinear motion shown in Fig. 2.

9364 Charles R. Cross—Direct.

To sum up briefly what I have said, I can see no possible room for doubt from the inferences which I have drawn from the appearance of which Fig. 2 is a diagram, confirmed still further by the appearance when the vibration is spread out, as shown in diagram Fig. 1, that these results demonstrate with perfect clearness the existence of electrical oscillations in the primary circuit of the apparatus under consideration.

Q. During the said tests, did you observe in the Braun tube energies representing oscillations in the antenna or radiating circuit of the apparatus? If so, explain briefly.
9365 A. I did. I have indicated in Fig. 3 of my drawing the general character of the phenomena which I observed. Fig. 3, to which I am referring, shows the outline of the envelope of the vibrations of the cathode beam, when the Braun tube was connected with the antenna circuit. This line which I have drawn is, as I have said, the enveloping surface of the vibrations, and the interior portion is bright, and most so at the envelope; as would, of course, be expected, since it is there that the vibrations reverse their direction. This curve shows first a gradual building up of the antenna current, and afterward, at a later stage, its dying away, the vibrations in the first part of the curve growing stronger
9366 and stronger and in the latter weaker and weaker. The increasing portion of the envelope represents the gradual building up of the oscillatory current in the antenna, under the influence of electrical oscillation in the primary circuit. The diminishing portion, falling to zero, indicates the condition of gradual diminution of the oscillatory currents in the antenna as these decay, being no longer under the influence of oscillations in the primary.

In my opinion, we have in the building up and falling off of the antenna current in the apparatus under con-

 9367

sideration a further very convincing and beautiful proof that there are electrical oscillations in the primary circuit; for it is only by the cumulative action of such oscillations that a gradual growth of the antenna current can result. Such oscillations in the primary are essential to a gradual building up of a secondary current, like that in the antenna. Perhaps I should remark, lest I should seem to overlook the fact (although it has been explained at length by Mr. Chaffee in his deposition), that the scale of Fig. 3 and the appearance of the plate which it represents are not uniform, as they have been in the other cases which I have considered. 9368

With so long a duration of the total current as is necessarily employed in the study under consideration, the discharge of the auxiliary condenser which electrifies the electrostatic plates in the Braun tube has proceeded so far that the deflection of the cathode spot is no longer proportional to the time, and in fact, the very slow oscillation which is present in that circuit has begun to reverse its direction.

By Mr. Peters: I offer in evidence the pencil drawing produced by the witness, containing his figures 1, 2, 3 and 4. The same is marked "Plaintiff's Exhibit, Cross Drawing, Figures 1, 2, 3, 4." 9369
(Plaintiff's Ex. No. 67D.)

By Mr. Farnsworth: The same objection is made to this exhibit as to the entire testimony, not so much in respect of the testimony itself as in respect to the incompetent and immaterial alleged defendant's apparatus, tested by Dr. Chaffee, on which the deposition of this scientific witness is based.

Q. The defendant in this case is alleged to have made certain *ex parte* tests of its apparatus at Washington

University, Seattle, and it appears that in those tests a Braun tube was employed, having outside electrostatic deflecting plates, and that those plates were connected directly across the spark gap of the wireless transmitter under test. In your opinion, are any conclusions as to the oscillatory character of the primary circuit based on observations made in those tests of any value whatever? A. In my opinion they are not.

I have been acquainted with the Braun tube practically ever since it has been used, both in its earliest form when intended only for use in an electromagnetic
9371 field and also with later tubes furnished with internal plates for use with an electrostatic field.

I have realized for a very long time that external plates with such a tube could not give any true indication of the characteristics of electrical oscillations, and I recollect very distinctly that before I possessed such a tube with potential plates I had thought of employing external plates, but dismissed the idea on consideration of the conditions under which such plates would operate.

I was well aware of the fact that the varying potential of external plates would not induce a correspondingly varying field within the tube, but that internal plates were necessary for such studies.

9372 The simple experiment mentioned by Dr. Chaffee which I have seen, in which the terminals of an influence machine were connected with external plates which were charged from it, the cathode beam meanwhile showing no deflection, is an all sufficient indication of the absolute lack of value of any studies of this character made with a tube furnished only with outside plates.

Apart from this, the location of the Braun tube in defendant's experiment with relation to the spark gap would also prevent its giving any true indications of the condition of the current in the primary circuit.

 9373

Q. Were you present and did you hear the testimony given by Dr. Chaffee regarding the tests of July 3rd and 4th? A. I was present and did hear the testimony.

Q. Do you agree with the conclusions reached by Dr. Chaffee and set forth in his testimony regarding those tests?

Objected to as vague and indefinite.

A. I do.

Q. State briefly the circumstances under which you attended the tests? A. Some time in the week pre-
ceding July 3rd I was called up on the telephone from 9374
New York by Mr. L. F. H. Betts, who asked me if I was free to witness certain tests to be made at the Cruft Laboratory on the 3rd of July and perhaps continuing; the telephone line acted very badly that afternoon, so that I could not understand all that he said, but in a letter received the next morning, he stated more particularly what the tests were, and that he would cause to be sent me a more detailed statement; this I received from Mr. Waterman, although it was somewhat delayed in the mails.

What I was asked was to witness with my own eyes
certain experiments to be tried by Mr. Chaffee with cer- 9375
tain apparatus which was described to me in a description substantially identical with what is shown in Exhibit Chaffee Diagram 1.

Also it was desired that I should witness certain experiments with a Braun tube having external plates.

I was also informed that I was likely to be asked to give a brief deposition as to what I should witness in these tests.

9376 Charles R. Cross—Cross.

CROSS EXAMINATION.

Questions by Mr. Farnsworth.

XQ. Professor Cross, will you please calculate the number of oscillations in the dummy antenna of Dr. Chaffee used in his test of the Simpson Mercury Valve Transmitter? A. 60, I think.

XQ. That was a decrement of what, Professor Cross? A. I don't recollect; I am simply taking that as his figures.

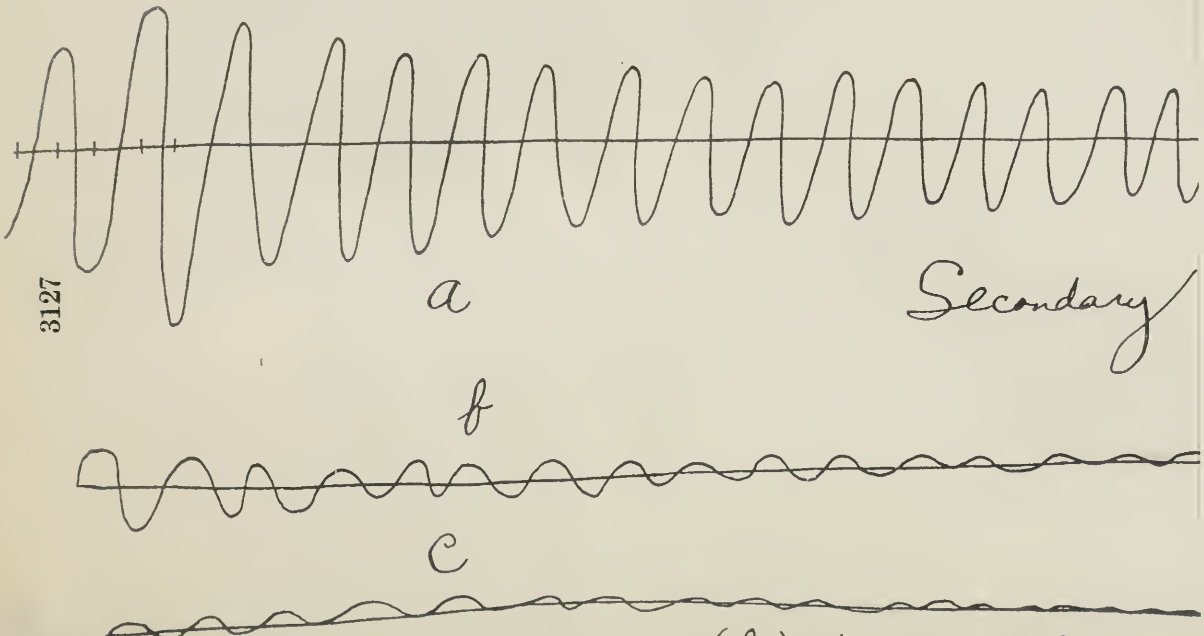
XQ. Will you please make a sketch showing the cur-
9377 rent action in Dr. Chaffee's tests in both the spark circuit and the dummy antenna circuit? A. You mean showing simultaneously, that is, what you want is the long train of waves?

XQ. Yes; and in the spark circuit, too, above it (reproduced opposite).

Mr. Farnsworth: I offer in evidence the diagram made by Professor Cross, being Defendant's Exhibit Cross Diagram No. 4. (Defendant's Ex. No. 52.)

XQ. Will you please calculate the number of oscilla-
9378 tions in the dummy antenna employed in the Chaffee tests with the Simpson Mercury Valve Transmitter? A. I have not the formula in mind for this work, and have not qualified as an expert in the mathematics of the subject.

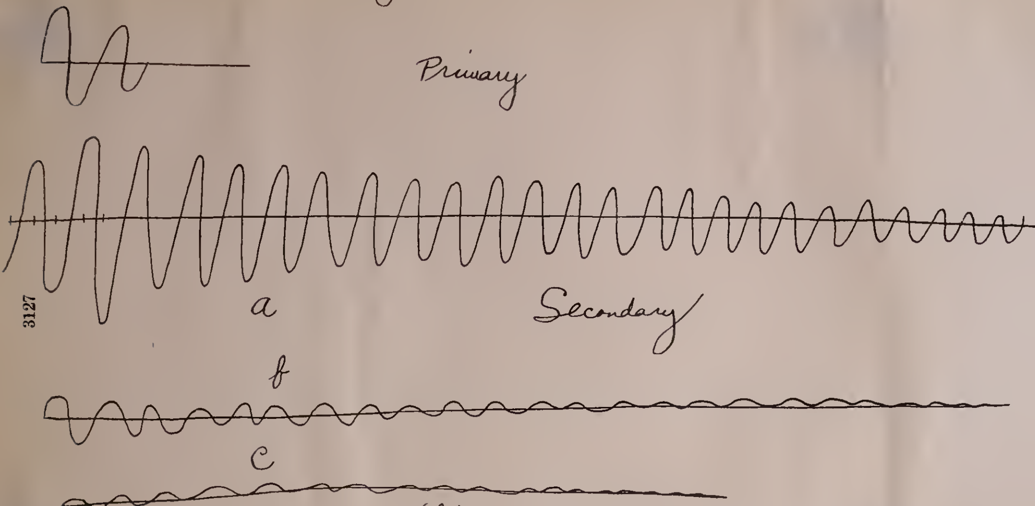
XQ. What kind of ionization took place in the Braun tube used by Dr. Chaffee during his tests with the Simpson transmitter? A. I don't know that I am quite sure that I understand the question. What actually was employed was, as I have said, the cathode stream or stream of cathode rays, which proceed from the cathode



(b) is continuation
(c) " "

Deft Ex 52

Def's Exh Cross Diagram No 4



(b) is continuation of (a)
(c) " " " (b)

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of, in this case, a high voltage storage battery. These ions, if you prefer to use that term, are negative corpuscles or elementary charges of negative electricity, whose mass, as I have had occasion to say in a preceding answer, is about 1/1800th that of the hydrogen atom. Under the influence of very high electrification, these corpuscles or electrons stream forth at right angles to the cathode or negative electrode.

XQ. Professor Cross, will you please enumerate the various occasions on which heretofore you have acted as expert witness or expert affiant for the plaintiff Marconi Company? A. I don't know that I can give them 9386
all, but I will name all those which I can recall. They were all of them a long time since.

If I recollect rightly, I gave testimony for the Marconi Company in a suit between it and the company owning a patent to Professor Dolbear.

I also recall that I gave testimony for the Marconi Company in a suit brought under the fundamental Marconi patent against a company holding the De Forest patents.

I also gave testimony for the Marconi Company in an interference suit in the United States Patent Office, in which the subject matter was the magnetic, detector. 9387

There was some other suit, if I recollect rightly, in which I gave either a deposition or affidavit, but I have forgotten when or against whom the suit was brought.

These are all that I can recall. I don't remember that I have acted for the plaintiff, in any way, I should think, for eight or ten years.

By Mr. Peters: It is noted that the spark gaps used by Dr. Chaffee and sealed by him at plaintiff's request have been offered in evidence by the defendant and are delivered into the custody of the Court as represented by the Notary, and are not now with the transmitter.

Deposition closed.

Adjourned *sine die*.

(Signed) Charles R. Cross.

(Plaintiff's Replying Evidence.)

9389 Roy A. Weagant, recalled as a witness on behalf of plaintiff, testified as follows:

Q. (Mr. Betts) Mr. Weagant, you have already been sworn to testify in this action? A. Yes, I have.

Q. What, if anything, did you have to do with the disposition of the Simpson mercury valve transmitter which was loaned the Marconi Company by the defendant company last April? A. The apparatus was received at the shops of the Marconi Company here in Seattle, and from that time on it was in my control, and whatever was done to it was done under my direction. At the conclusion of the procedure in court that apparatus was carefully boxed
9390 up and shipped to the laboratories of the Marconi Company at Aldine, New Jersey.

Q. About when was it received at the factory of the Marconi Company at Aldine, New Jersey? A. It was about the 17th of May, I think—I think that was about the time, the 17th.

Q. And what was done with the apparatus after it was received at the Marconi factory in New Jersey? A. It was unpacked and set up and connected to the usual dummy antenna, and a number of preliminary tests were made on it.

Q. For what purpose? A. Well, to see that it was in good working condition and had not been damaged in any way in transit, and to become familiar with its connections and operation, and to make various tests on it.

Q. You made tests yourself on that apparatus during that time it was at the factory? A. Yes, I made a few informal tests. I had it carefully connected up and tuned. I tested and checked the tuning, measured the various wave lengths, different circuits, and then I operated it with two different generators. One generator was the 120 cycle generator which we had received from the Kilbourne & Clark Company, and which had been shipped 9392
from here, and the other was a 500 cycle generator manufactured by the Crocker-Wheeler Company of New Jersey.

Q. Now, what do you know about the shipment of that apparatus to the Cruft high tension laboratory at Cambridge, Massachusetts? A. Well, it was packed up under my direction and shipped to Dr. Chaffee at the Cruft laboratory, and arrived there, I think, about the 4th or 5th of June.

Q. What generator did you send up there with that apparatus? A. The Crocker-Wheeler 500 cycle generator.

Q. Why did you send the Crocker-Wheeler 500 cycle 9393
generator? A. I was instructed to by Mr. Waterman.

Mr. Betts: You may cross examine, Mr. Farnsworth. I wish to explain to the court that I have called Mr. Weagant to connect up the receipt of this apparatus to the shipment to the Cruft high tension laboratory, and at a later date I will call him in regard to any tests that he may have witnessed at a subsequent time.

The Court: I understand.

9394 F. N. Waterman—Recalled—Direct.

Mr. Farnsworth: No cross examination.
(Witness excused.)

F. N. Waterman, recalled as a witness on behalf of Plaintiff, testified as follows:

Q. (Mr. Betts) Mr. Waterman, you have already been sworn and examined as a witness in this case? A. I have.

Q. Have you now had an adequate opportunity to familiarize yourself with the apparatus and mode of operation of defendant's Simpson mercury valve transmitter? A. I believe I have.

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Q. Will you please now compare the apparatus of defendant's Simpson mercury valve transmitter with the transmitting apparatus illustrated in Fig. 1 of the Marconi patent in suit, No. 763,772, and described in the specifications thereof, and point out to the court what, if any, similarities, or dissimilarities you find to exist between the two transmitting apparatus. A. I find that the Simpson mercury valve transmitter consists of two circuits having reference to its essentially wireless telegraph part, like the apparatus which is described in the Marconi patent and illustrated in Fig. 1 thereof. This transmitter comprises an open radiating circuit, which consists of the elevated wire A in the diagram——

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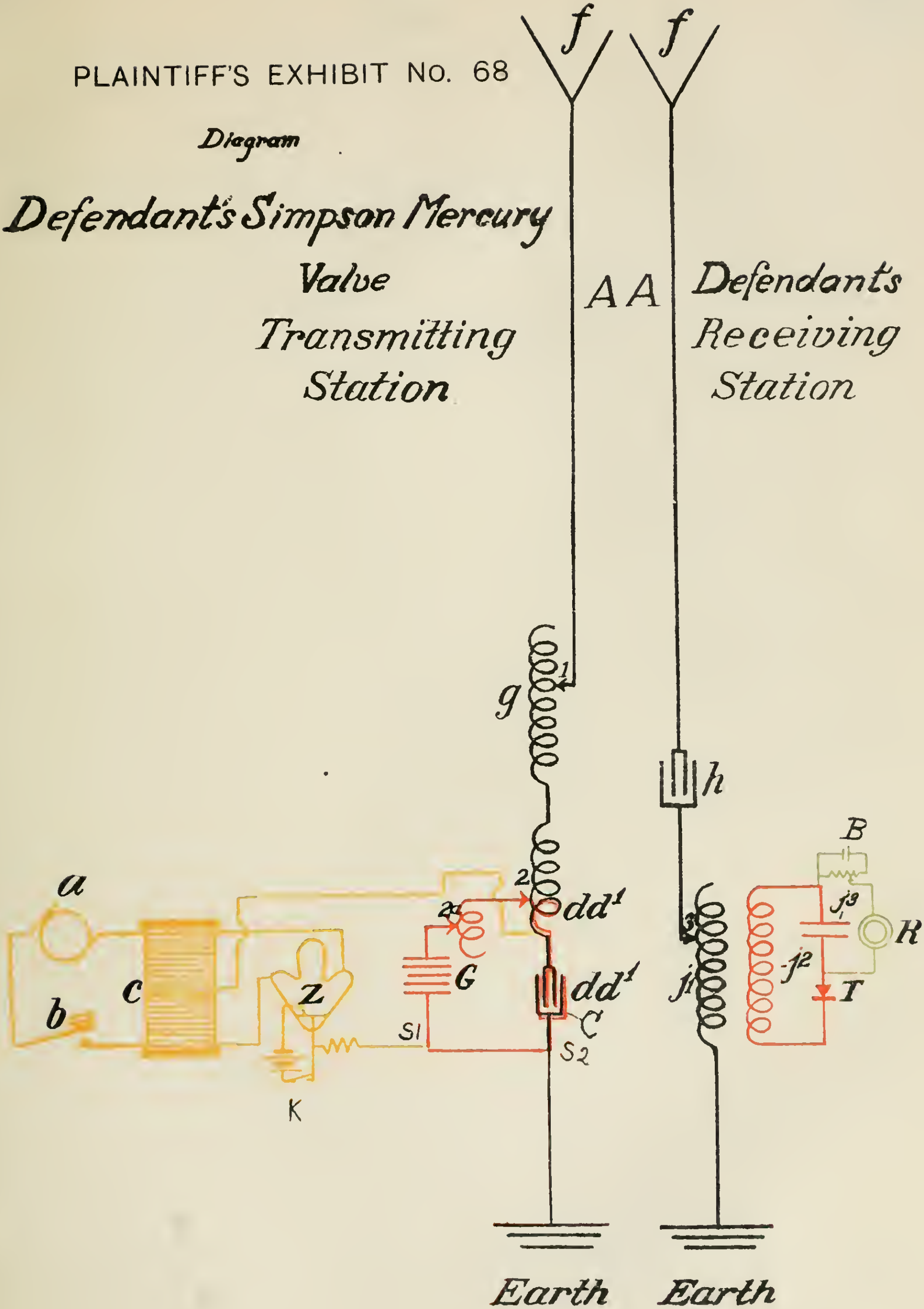
The Court: Let us get that marked in the record.

Q. Have you had a diagram prepared illustrating diagrammatically this transmitter? A. I have, entitled "Plaintiff's Exhibit No. 68" (Reproduced opposite). (Witness marks diagram "Plaintiff's Exhibit No. 68.") Exhibit No. 68 is a diagram of defendant's Simpson mercury valve transmitting station and receiving station. The

PLAINTIFF'S EXHIBIT No. 68

Diagram

Defendant's Simpson Mercury Valve Transmitting Station



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transmitter comprises two circuits, one the open, radiating circuit, which consists of the elevated conductor A, a suitable capacity area f at the top, a coil g, another coil dd1, a condenser, which I will give the letter C, which, for a reason I will explain, is also marked dd1, and the connection to earth. This is the circuit from which the energy is radiated to be received at a distant station.

Q. That circuit is colored how? A. That circuit is colored on this diagram in black throughout. There is a second circuit, the path of which I have indicated in red throughout. This circuit consists of the condenser c, a small portion of inductance dd1, 9404
a second inductance marked 2a, a spark-gap G, and the necessary connections to complete the circuit. This cirsuit thus shares in common with the open circuit a portion of the inductance dd1 and the condenser C. This latter circuit is a circuit which does not radiate energy, but into which energy is sent by the power supply system, and in which it oscillates through the breaking down of the spark producer or gap G, and by that oscillation it transfers its energy to the radiating circuit.

Q. How is the power circuit shown in plaintiff's exhibit No. 68, in what color? A. The power circuit is throughout indicated in yellow.

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Q. What does z in this exhibit indicate? A. It indicates that portion of the power circuit which is constituted by the mercury valve, and is a mere diagrammatic indication of that valve.

Q. What do a, b, and c diagrammatically illustrate? A. a merely indicates a suitable generator, b is the operator's key, and c is the transformer, by which the alternating current from the generator a is raised in pressure to a sufficiently high voltage for the charging of the condenser C. This charging takes place, as I will

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explain, through the mercury valve, so that the condenser is always charged in one direction.

Q. Will you please continue the comparison of this transmitter with that of the Marconi transmitter? A. This exhibit No. 68 I have caused to be drawn with the same colors as are used in plaintiff's exhibit No. 1, which has heretofore been referred to in the case. The open antenna or radiating circuit of Figure 1 of the Marconi patent similarly consists of the elevated conductor A, a suitable capacity area f, the coil here marked 1, but which in the Marconi patent is marked G, the coil
9407 d1, and the earth connection. This circuit corresponds part for part with the circuit antenna of the Simpson mercury valve transmitter. Similarly, the Marconi Fig. 1 has the local or closed primary oscillating circuit. This consists of a condenser e, in which the energy is stored, an inductance coil d, and a spark producer G. This condenser is charged in a similar manner from the yellow circuit on exhibit 1, including the part c, which is the transformer, source of current a, and the operator's key b, this also being merely a diagrammatic indication.

The Simpson mercury valve transmitter, I find, has its open or secondary circuit characterized by the fact that it is a radiator of energy. Energy imparted to that
9408 circuit in any way and once set in oscillation oscillates more and more feebly as its energy is radiated off into space. The same is true of the antenna as it is shown and described in the Marconi patent. The primary circuit is composed of a storage or reservoir device, namely, a condenser C in the Simpson mercury valve transmitter, a suitable adjusting means, which is the small coil 2a and the spark producer G. This circuit does not radiate energy, like the circuit d e G of Fig. 1 of the Marconi patent in suit. It is merely a circuit to which energy is imparted in a quiescent or static form, and which acts

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as a trigger to set the whole system in oscillation. When the gap G breaks down the energy which is resident in the condenser as a charge is suddenly released and oscillates in this circuit G d e of Fig. 1 of the Marconi patent in precisely a similar manner. The condenser C of exhibit No. 68 is charged, and upon the breaking down of the spark producing gap the energy is set in oscillation through the red circuit of exhibit No. 68, representing the primary circuit of the Simpson mercury valve transmitter. The Marconi patent gives a large number of illustrative examples of precisely how the apparatus may be constructed. These are found tabulated on page 4 of 9410
the Marconi patent, and the specific construction of the elements is quite fully described on pages 3 and 4. The first four of these examples, in fact the first five of these examples on page 4 are characterized by a very large ratio of capacity to inductance. That means that a large reservoir is provided at C in exhibit 68, at e in Fig. 1 of the Marconi patent, as shown in exhibit 1.

2. In the form of what sort of an apparatus? A. In the form of what is called a condenser, being simply separated plates of metal, separated by some insulating material, as glass, air or mica. The particular insulating material which may be used is not specified in the Marconi patent, although air is suggested as desirable. The 9411
material employed in the Simpson Mercury Valve Transmitter for separating these electrodes or armatures of the condenser, is mica. The energy in such an oscillating circuit is stored first as electrical pressure. There is a high pressure produced across the terminals of the condenser, and a correspondingly large amount of energy stored in it. That energy, upon being set into oscillation by the spark producer, goes into the form of electrical momentum, kinetic energy, and then is momentarily stored in the inductance. If the inductance of this cir-

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cuit is small and the given large quantity of energy is to be rendered kinetic in it, evidently the current must be large; so that this Simpson Mercury Valve transmitter is characterized by a large condenser and relatively small inductance, and hence a large current flowing in this primary circuit across the spark gap G, or oscillation producer.

In these first five examples given on page 4 of the Marconi patent precisely similar conditions exist. The coil which constitutes the inductance of the circuit is made up of but a single turn—a single turn of wire. Simi-
9413 larly in the Simpson Mercury Valve transmitter the coil by which association is made with the antenna circuit is made up of a little less than a single turn of the wire. Mr. Simpson states from a half to three-quarters, if I remember rightly, and that is what I have found.

An additional inductance is included in this Simpson Mercury Valve transmitter, making a total inductance probably somewhat larger, but only a trifle larger than that set forth in these examples in the Marconi patent.

The two circuits are, therefore, circuits of like kind in that they are circuits containing capacity, inductance and the oscillation producing means. They are alike in their proportioning, in that they are circuits having a
9414 large ratio of capacity to inductance, or as we say technically, a large C over L—C standing for capacity, and L for inductance. The specification defines the circuits which Marconi used in a general way on page 2, beginning with line 12 of the patent, where Marconi says:

> "My experiments have demonstrated that the best results are obtained at the transmitting station when I use a persistent oscillator, an electrical circuit of such a character that if the electro motive force is suddenly applied to it and the current then cut off the electrical oscillations are set up in the circuit which persist and are maintained

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> for a long time—in the primary circuit, and using a radiator, i. e., an electrical circuit which very quickly imparts the energy of electrical oscillations to the surrounding ether in the form of waves in the secondary circuit.''

That is the way in which Marconi defines the two circuits. One is a circuit in which if energy is placed it must go on oscillating forever, or until it is consumed within itself,—because it cannot radiate. The other is a circuit in which the oscillations, while, theoretically going on forever, quickly impart their energy to the surroundng ether in the form of waves. One circuit, in other 9416
words, is the circuit that produces the ultimate useful effect, and that is the secondary radiating circuit. The other is the circuit which produces no such effect at all, but is the circuit to which the energy is originally imparted.

If we take this closed circuit of the Simpson Mercury Valve Transmitter——

Q. Which is shown in what color? A. Which is shown in red in exhibit 68—and set that circuit in oscillation, that circuit would oscillate forever were it not for the energy consumed within itself, if we took the spark gap, which consumes energy, out, for example. The Marconi patent shows the largest possible ratio of C over L in 9417
the first five illustrative examples which it gives, and there are certain distinct advantages from the wireless telegraphic point of view, in doing that, which I will later refer to—there are also, unfortunately, some disadvantages in doing it. At any rate, it is a selected quantity—a matter of selection applied by the designer as to how great he will make the capacity which he has in his primary circuit with reference to his inductance. But the question might arise: ''Why not make them both large?'' The reason is that if the wave length to be

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transmitted is fixed, then the product of the capacity by the inductance is fixed, because it is that product that determines the rate of oscillation, or, as we say, the wave length. As the object is almost invariably to design a transmitter for a fixed wave length—the product of C by L is fixed—fixed by law, for example, in the case of ships. The designer determines what ratio he will make those quantities—the product is fixed, but the ratio he can determine—he may use a big inductance and he may use a small condenser or a larger condenser and smaller inductance. The Marconi patent sets forth in five out
9419 of the six examples of transmitters which it gives, the largest possible ratio of capacity to inductance, and that is substantially what I find to be the fact with the Simpson Mercury Valve transmitter.

Q. You mean by that that they use in both instances a large condenser capacity as compared with a small inductance? A. Yes. In other words, the Simpson Mercury Valve transmitter has the two sorts of circuits which the Marconi patent describes. It has in those two circuits the same physical elements and it has these same physical elements proportioned in the same way.

Looking for a moment at the power circuit by which the condenser is charged—although I do not understand
9420 that this determines any part of the essential character of the transmitter in either case—but coming to the apparatus for charging which is shown in the patent and the charging part of the Simpson Mercury Valve Transmitter I find in the Marconi patent there is described a so-called induction coil—induction coil c, which is diagrammatically illustrated as excited by a battery. It is characteristic of such an induction coil which, in order to give high potentials must have its currents rapidly interrupted, that the impulses in one direction are enormously greater than the impulses in the other direction.

The result is that if a wireless telegraphic primary circuit such as G e d, figure 1, of Marconi patent has its condenser charged by such an induction coil, the spark will take place always in one direction. That is to say, there is unidirectional charging of the condenser. The reason for that being, as just stated, that the impulse due to the breaking of the primary current of the induction coil is many times greater than the impulse due to the making of that current. Hence the Marconi patent shows an arrangement in which the condenser is unidirectionally charged; the breaking down of the gap
always occurring in one direction, as, merely for illus- 9422
tration, let us say from the top to the bottom terminal of the gap. The Marconi patent expressly states that that is merely for illustration. It says, for example, at the bottom of page 1:

> "It is obvious that instead of the induction coil and associated parts for producing electrical waves or oscillations, I may use any other proper means for producing such electrical waves or oscillations, such, for example, as a generator of alternating currents."

Referring to Exhibit 68, the Simpson Mercury Valve
Transmitter has a generator of electrical currents, a, 9423
which excites the transformer c, which is merely a specialized form of induction coil operated by an alternating source. In order that the condenser may be charged always in the same direction and the spark gap thus always breaking down in the same direction, there is inserted in this condenser charging circuit, a mercury valve z, which is proportioned to allow the current to go through it only in one direction. In fact, it is a device which does not allow the current to go through it at all unless it is kept alive in some way, and that is done by a small direct current circuit, shown as k just below the mercury valve z.

9424

Q. That is what has been called the "Keep-alive Circuit"? A. That is what has been called the "Keep-alive Circuit," and it acts by keeping an arc, that is a flow of current producing the bright, greenish light always in the tube; that light being merely characteristic of the flow of current through mercury vapor.

Before this Simpson Mercury Valve Transmitter can be used, therefore, you have to first start the keep-alive circuit, and that is done by tipping the valve itself a trifle until the mercury runs from the little pocket in the bottom and comes in contact with the little keep-alive ter-
9425 minal in the side. Then upon restoring it to its vertical position it lights up and the tube is conductive, but it is only conductive in one direction. The two outer terminals of the secondary power transformers, c, are taken to two electrodes which are fastened in arms sticking out from the body of the mercury valve, and the conductivity of that valve is such that if an impulse is produced by the generator in the coil c, for example, up so as to tend to come out on the upper wire, that current can flow over to the right hand arm of the tube z, thence through that pool of mercury at the bottom of the valve; thence to the condenser, charging the condenser; thence back to the middle point of the transformer.

9426 Q. c? A. c. Upon the next alternation the current, of course, would be in the opposite direction, that is, on the former assumption it would be therefore in a downward direction and it would flow out of the bottom line into the left hand arm of the rectifier; thence through the pool of mercury to the bottom of the condenser again. So that, no matter which way the current is flowing, the condenser is always charged in the same direction. That is another point in common then between the Simpson Mercury Valve Transmitter and the particular arrangement that is shown, for the sake of illustration, in the Marconi patent, in the induction coil exciting means.

Q. In referring to that you have reference to plaintiff's exhibit No. 1, previously introduced in evidence? A. Yes. This figure to which I am referring under the head of "Marconi No. 763772-'00 Transmitting Station," being a reproduction of figure 1 of the Marconi patent, but in colors, the power circuit being in yellow or orange.

Q. I think you described that; I merely wanted you to show what exhibit it was, because, while you described the diagram you have not referred to the particular exhibit, of which numbers it is "Plaintiff's Exhibit No. 1." A. I am merely identifying the colors on this with the drawing of the Marconi patent. The colors are yel- 9428
low, red and black, as already noted. So that the Simpson mercury valve transmitter is a very literal following of the Marconi patent as to the sort of circuits which it has, the association of those circuits, the proportion of those circuits and the unilateral, or one direction charging of the primary circuit.

The Marconi patent describes also means for adjusting these two circuits. Thus, there is shown at 1 a means of adjusting the frequency or wave length of the antenna circuit; the condenser, which is shown at e of Fig. 1 of the Marconi patent, so that the time period of both these circuits may be adjusted. Looking at the Simpson mercury valve transmitter, I find that the antenna circuit is 9429
similarly adjustable at Fig. 1 on this coil g, and that the secondary circuit is adjustable at the point 2, at the point 2a and in the condenser C. Thus, the two circuits are, in the Simpson mercury valve transmitter as in the Marconi transmitter, adjustable; each circuit being provided with means whereby its exact time period can be determined. The Marconi patent describes the tuning of the two circuits together; that is to say, their adjustment to resonance with one another. I have already read that several times, and I take it I need not repeat it.

9430 F. N. Waterman—Recalled—Direct.

Q. No, I think the court is quite familiar with that.

The Court: Yes.

A. (Continuing) These means in the Simpson mercury valve transmitter are corresponding means to those described in the Marconi patent and are used for the same purpose, and I find that their use for that purpose is exceedingly essential in this Simpson mercury valve transmitter; that is to say, the tuning or adjustment of this Simpson mercury valve transmitter is very critical. If the two circuits are thrown out of resonance with one
9431 another by a very small amount the efficiency, that is, the amount of energy radiated is very greatly reduced; so that it is very necessary that the two circuits should be exactly in resonance within the possibility of accuracy in such adjustments. That is, they must be as nearly as possible in resonance, and if they are thrown just a little out from resonance—ten per cent out, for example, the reduction of energy is very great. So that these circuits are such circuits as are described in the patent, proportioned and adjusted in the same way and with the same relation with one another, for the same purpose. Further, comparing the two structures, I call attention to the fact that the specific connections of the two are
9432 different. In the Marconi patent, Fig. 1, we have two circuits not physically in contact with one another. The primary circuit has a loop of wire, d, which is in very close proximity to, but not coincident with a couple of turns, d1 in the antenna circuit. Figures 3 and 4 show one physical structure that those may have, as described by Marconi. In the Simpson mercury valve transmitter we have what is a very common alternative for that, namely, the use of the same conductors—conductors in common. That is simply, in other words, bringing the

turns closer together—still closer together by making them coincident.

I understand there is no controversy on that point in this case, because Mr. Pickard has agreed that that is a full equivalent.

Q. Now, those two turns of the Marconi transmitter, what kind of a transformer is that called? A. That is often spoken of as an inductive coupling or a two coil or inductive transformer. The English call it a jigger. The type where the wire is actually common is called auto-transformer, or as the English call it, an auto-jig-
ger. There is no controversy on this point as to the full 9434
equivalent of these—at least that is what I understand from the record.

Mr. Pickard, in his diagram G. W. P. 4, illustrated equivalence of three different methods of associating two circuits, and he illustrated a fourth method, which he said was without utility.

He says, on page 519 of his testimony:

> "I can illustrate this matter of coupling or linking by three sketches, these forming figures 1, 2 and 3, of my sketch 'G. W. P. 4'; the first method or coupling is that which we call inductive, and which I have shown in figure 1. Here two circuits,
> which I have shown diagrammatically as A B and 9435
> C D are linked or coupled together through the inductive relation of two coils L1 and L2. If an electrical current is caused to flow in any manner in circuit A B L1, it will set up magnetic lines of force in the space around the coil L1, and some of these lines will thread through or link themselves with the coil L2, and will induce, as we say, currents therein. That is to say, currents in the second circuit C D L2. That method of linking together the two circuits,—the method shown in figure 1,—is the inductive coupling. Another method of coupling is the electrostatic or capacity

9436 method of coupling, which I have shown in figure 2. Here, two circuits, A B C and B E C are linked through a common element C, this element being a condenser. If, in any way, a current is set up in circuit A B C, and if this current is an alternating or varying current, such as the currents which we always have to consider in radio communication, a varying potential will be set up in the plates of the condenser C, and a current will be transformed to circuit D E C."

Then he discusses the magnitude of the coupling, which I shall skip for the moment, although I will refer
9437 to it later.

Then further down, on page 520, he says:

> "The third method of coupling, which I have shown in figure 3, is the so-called direct, or, as it was once called, auto-transformer coupling. In figure 3, the circuit A B is linked in the circuit D E by the common element L, which is an inductance, or coil of wire. Electrically, I may say, this form of coupling does not differ in any way from the coupling of figure 1. The coils L1 and L2 may be combined in a single coil L, as I have shown in figure 3, and the result is the same. In fact, in the ordinary use of transformers, in alternating the current power distribution, and dis-
> 9438 tribution of power for lighting, such a transformer or coupling as I have shown in figure 3 is of more use, and it is customarily called an auto-transformer."

So I understand there is no controversy as to the fact that the use of a common bit of wire to form a common turn dd1, of Exhibit 68, does not distinguish it in any way from the use of two bits of wire close together to form those turns—the structure is substantially the same, and the result is identical.

 9439

Q. What does he state as to the use of the condenser common to the two? A. I am going to read about that in a moment—I have already read part of it.

So that so far as the association of the two circuits by current flowing in coils is concerned, dd1, the association of the two circuits is identical.

Now, there is a further detailed difference of connection in the Simpson Mercury Valve transmitter in that the condenser C is also in the ærial circuit as well as in the local circuit. That, as I just read, is another one of the three equivalent forms of coupling which Mr. Pickard has described. 9440

Q. Where does it show in G. W. P. 4? A. It is shown at figure 2 in G. W. P. 4, where the coil or loop A B is coupled to the loop D E by the common connection therein of the condenser c. Obviously the circuits would be equally coupled if one of the circuits was of a radiating form instead of a closed form.

Mr. Pickard says this is one of the three equivalent forms of coupling. He says, on page 520:

> "Similarly, in the electrostatic coupling, which I have illustrated in Fig. 2, the tightness of this association or linkage depends upon the relative magnitude of the condenser c, and any capacity which exists in either circuit a b c or d c e. Another way of stating that would be: If this condenser c is very large indeed, the coupling is quite loose. If the condenser is very small, the coupling is quite tight, although the exact amount of the coupling would be determined in other elements, which I have not shown in these circuits, for the sake of simplicity." 9441

In other words, what Mr. Pickard says there, and what the universally recognized fact is, is that we may couple circuits by using an electrostat or by using a common condenser. But there is a very confusing dis-

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tinction which exists between treating of condensers for coupling and treating of inductance. For example, if in exhibit 68 we increase the amount of the coil dd1, which is common to the two circuits, or increase its size we would tighten the coupling. We would make the association more intimate, but if we increased the size of the common condenser we would make the association less intimate. The reason for that might be very crudely illustrated in some such way as this: if we fasten together two things by a very stiff spring—two balls, for example, by a very stiff spring they will remain together,
9443 like two dumb bells, for instance, but if the spring were more elastic the balls could roll about the floor, practically independent of one another—they could not move to indefinite distance apart, but they would not be strongly drawn together.

Now, the big condenser acts like the weak spring and the small condenser acts like a very stiff, rigid spring.

That is what Mr. Pickard means, and he is quite right when he says that if we put as a common element between two circuits the condenser marked dd1 in No. 68, also marked C, which is very large, we practically do not influence the coupling of those circuits, at least we influence it very little; but if they were very small, then
9444 it would be an important factor in the coupling. In other words, his condenser c of Fig. 2——

Q. G. W. P. 4? A. G. W. P. 4 (Vol. 2, p. 435), will more intimately associate the two circuits if it is a very small condenser, but it will very loosely associate them if it is a very large condenser. So, in putting this condenser into the ærial——

Q. Of what structure? A. Of the Simpson mercury valve transmitter, Mr. Simpson has simply put in a small additional element of coupling, when he has put in a large condenser.

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I find, for example, in using this, it practically does not alter the operation of the transmitter at all, to simply connect the ground lead above the condenser, so that the condenser then is only in one circuit—it calls for extremely little readjustment.

In other words, the presence of the condenser C in the antenna circuit, as well as in the local circuit, has substantially no effect whatever on the operation, and in so far as it has an effect it is merely, as Mr. Pickard states, as equivalent to a little more or less wire in the coil.

Thus, the Simpson mercury valve transmitter has the 9446
elements of the Marconi patent, proportioned as described there and associated, if not literally in the same way, by ways that are precisely equivalent.

And as to this last matter of the equivalents of the association, I do not understand that there is any controversy.

Now, a question which could arise is "How close is this association?" The Marconi patent describes a number of different specific installations as to which it has been found—I have found personally by calculating and by tests of the circuits practically constructed—that the coupling runs, as we say, from five to about
twenty per cent in those various illustrations. One of 9447
them is as low as five and one of them as big as twenty, and the others lying between.

Q. Were those tests and calculations made by you, made prior to the beginning of this suit, or since this suit was started? A. They were made—both the calculations and the tests I made several years ago, and I testified about them in the case before Judge Veeder.

The coupling of the Simpson mercury valve transmitter, as shown in Exhibit 68, has been shown to us by Mr. Kolster's report and I find that it is about seven and three-quarters per cent. In other words——

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Q. Seven and three-quarters per cent coupling? A. The coupling is seven and three-quarters, as against the minimum of five in the Marconi and the maximum of about twenty. Therefore, the circuits are not only like circuits and associated in like manner, with like relations of constants and like tuning, but the coupling is also of exactly the same order.

The spark gap which is illustrated in a very diagrammatic way in the patent, Fig. 1, and is not anywhere in the patent specifically defined. It is referred to on page 1, line 26, as a spark producer; on line 45 as a spark pro-
9449 ducer; on line 84 as "a spark producer or other electrical wave or oscillation producer"; and is, so far as I notice, nowhere more specifically defined. It is referred to on page 2, line 30, as a spark gap.

The oscillation producing means which is found in the Simpson transmitter is a series of spark gaps, shown on g in Exhibit 68. That is to say, instead of showing one long gap, it is a number of short gaps in series; and, as Mr. Simpson demonstrated at the Kilbourne Clark laboratory at the former session of the court, the number may be altered by sticking into spaces provided, little copper band clips, so that we may have anywhere from one to all five in use. Mr. Simpson's statement, as I remember
9450 it, was that two was the minimum number employed in actual practice.

So that the difference between the two apparatus in this respect is merely in details of construction in the Simpson mercury valve transmitter which are not defined in the Marconi patent.

As a matter of fact, I may say, that this use of two or more gaps in this primary oscillating circuit is now universal. Indeed, this constitutes about the only departure which is noticeable—is about the only respect in which the transmitters of today do not literally follow the particular thing shown in the Marconi patent.

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Marconi simply says, "A spark producer or other means, or other electrical wave or oscillation producer". And ever since the production of that apparatus of the Marconi patent in suit. Mr. Marconi and others have been actively at work in the endeavor to improve, of course, all of the elements of the system.

And the use of two or more gaps in series is one of those features which is now quite universally used, either with the elements rotating or the elements stationary, and it makes no difference, except a matter of the merest detail which, nor is there any difference except in the
merest detail in the resulting operation, but the appar- 9452
atus is made more efficient when such a plurality of gaps is used, and for this reason: Looking to the Marconi patent, because the diagram is a little simpler, it will be seen that the spark producing means, g, is, as a matter of fact, necessarily associated with two circuits. That is one of the things that you cannot get away from—it is one of the unfortunate things. It is in the oscillating circuit d e g—it is also in the other circuit c g.

Q. When you say, "The other circuit" you mean——
A. ——the yellow circuit.

Q. The yellow circuit—I want to distinguish it in the record from the secondary circuit. A. The yellow cir-
cuit c g. And it is desired that the source of current c, 9453
shall charge the condenser C by a pressure so high that it breaks down the gap, but if the gap is broken down the difficulty is to keep the current of C from flowing directly across the gap, and really the only reason why such an arrangement works at all,—it ought to be, one would say from looking at it, quite inoperative—but the reason why it does work is because the rate at which things happen in the red circuit, that is the primary circuit, is so enormously greater than the rate at which they happen in the yellow circuit, that the yellow circuit, or the power

circuit, cannot keep up. If it did, why, these systems would be inoperative. Now, this tendency of current to get across gap g directly from the power transformer, appears to be less if the terminals are rotated rapidly with respect to one another—and there are several of them. It appears to be easy enough for the high frequency current to jump the gap, but less easy to jump a series of short gaps—and less easy for the power current to follow, and that is why the use of these improved gaps is universal today.

The court will remember that in the L. C. Smith Build-
9455 ing the gaps of the two transmitters consisted of very short gaps in series. I do not remember whether there was shown a rotating gap or not, but these are also used. The government station at Arlington uses a big rotating gap; the Marconi Transpacific Station at Belmas uses a rotating gap. In the smaller sets stationary gaps of two, three or four, as the case may be. gaps in series are the generally used thing today.

As I pointed out, the Marconi patent contains no explicit description of any form of gap which is to be used; it simply calls generally for a spark producer or other electric wave or oscillation producer; and that is element g in both of these cases; and the difference is only one of
9456 degree in any case.

Therefore, the two transmitters are alike, as I understand, in this respect also.

I might say that the reason why the plurality of gaps is better, is because they have got more surface, and cool quicker.

Q. This gap here that is represented by g in the defendant's Simpson mercury valve, is known in the trade as what kind of a gap? A. It is known as the quenched gap.

I think I have now covered all of the elements of

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structure of the Simpson transmitter, and compared it with the Marconi, except such obvious elements as the transmitting key and the like, which do not call for a description; and the Simpson transmitter is, as I find, made up of two circuits similarly situated, similarly adjusted, tuned, like ratios of inductance, and capacity charged in the same manner and discharged in the same manner to cause the same sort of operation.

Q. You pointed out, did you, Mr. Waterman, in answer to the last question, that there were means in the Simpson mercury valve transmitter for tuning or adjusting the two circuits together? A. I believe I referred 9458
to it, but I did not call attention to all the means.

Q. Please do so. A. The Simpson transmitter has a means of adjusting which is not shown on exhibit 68 at all, that is, it has the elements which I have already pointed out at Fig. 1 in the aerial circuit and at 2, 2a and C in the closed circuit, for the purpose of adjustment. Of course adjustments made at C are also in the aerial circuit, but, due to the fact that the condenser is so large, they have very little effect.

Now, the transmitter is provided on the face of the marble panel which carries the apparatus, with a rotating switch, and that rotating switch has connections
taken to the coil g and to the condenser C, and by the 9459
turning of that switch the operator may elect the use of any one of four different wave lengths. When that switch is turned, there is caused, automatically, an adjustment of the coil g, that is, moving of the point 1, Fig. 1, and an adjustment of the condenser C, thus keeping the two circuits, the black and the red, that is the secondary and primary, the radiating and the non-radiating, always in tune. And this is done because, as I have pointed out, the tuning of this transmitter is very critical. If it is out of tune a little bit its efficiency is

tremendously cut down because it relies on the resonance between the two circuits to get any good results. So that this transmitter is provided with the means of adjusting the circuits to varying wave lengths simultaneously, so that the wave length of the primary circuit is varied just exactly as the secondary circuit is varied, and if one is radiating six hundred meters, both are tuned to six hundred meters, and if the secondary is radiating five hundred meters both are adjusted to five hundred, and so for three hundred, and the same for the other waves. So that it is provided with more means of
9461 affecting the tuning than are shown in the Marconi patent, but they are means for the same purpose and used in the same way.

Q. Will you please now refer, Mr. Waterman, to defendant's exhibit No. 10, Mr. Kolster's Bureau of Standards report, in regard to the defendant's Simpson mercury valve transmitter, and, particularly also to Mr. Kolster's drawing, K-1, entitled, "Kolster Chart—Simpson mercury valve transmitter" (Vol. 2, p. 1227), and say whether or not that exhibit and Mr. Kolster's Bureau of Standards report shows that the open and closed circuits of Defendant's Simpson Mercury Valve Transmitter are or are not in tune? A. Defendant's Exhibit No. 10,
9462 being the report of the Bureau of Standards referred to by Mr. Kolster, shows numerically the fact of the exact tuning together of the two circuits to resonance, as I have referred to it in my preceding answer.

Kolster's chart, K1, is Fig. 1 of Exhibit 10 more or less accurately reproduced, with the points, however, from which the curves are plotted, omitted.

In this sheet 1 of the Kolster or Bureau of Standards report, which I will assume for the moment is illustrated in Chart K1, the curve 3 is the so-called resonance curve of the primary or red circuit of Exhibit No. 68, and the

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curve marked 1 is the resonance curve of the secondary or black circuit of exhibit 68, both taken when the circuits are coupled and operated normally.

The Court: How is that?

A. (Continuing) Both curves, 1 and 3, having been taken, according to the report, when the two circuits were coupled together and operating in their normal manner.

The other curves, 2, 4 and 5, represent various abnormal conditions which were tried, but 1 and 3 are the two which represent the normal operating conditions. 9464

The Court: The black and red—1 and 3—the black and red?

A. (Continuing) Corresponding to the black and red circuits in normal operative association.

The report states, page 3, line 6, "the curves and data on sheet No. 1 give the results of tests made with the apparatus connected in the usual manner as shown in the diagram of circuits on the sheet". These diagrams of circuits are at the right of sheet 1, of the report.

It further says, on the same page, line 12: "Curve 1
was obtained with the wave meter loosely coupled to the 9465
load coil in the artificial antenna circuit"; that is to the coil g of Exhibit 68.

And on line 22: "Curve 3 was obtained with the wave meter loosely coupled to the trigger circuit, but with the antenna circuit connected and properly adjusted".

He then goes on to explain that curve 2 was taken with the antenna circuit entirely disconnected; curves 4 and 5 were taken with the antenna circuit deliberately thrown out of tune.

And I might say, although I will explain it more fully later, that the court would be deceived if it paid any attention to the heighths of these curves. The point that is being here illustrated is their location with reference to wave lengths; and it will be seen that the primary circuit and the secondary circuit have their maximum effect on the wave meter at the same point. That is to say, their resonance curves show that the two circuits are in resonance with one another within the highest attainable degree of practicable accuracy.

Q. You mean by that that the uppermost point of curve 1 and the uppermost point of curve 3 are sub-
9467 stantially in a line? A. They are substantially in a line.

Q. In K1? A. (Continuing) And I call attention—as illustrating what I mean by "within the error of measurement" I called attention to sheet No. 1 of the report, where it will be seen that the dots from which the curve 3 is drawn, are at the top, scattered very irregularly. That is always the case in such a circuit containing a spark gap, and it is due to several causes. It is due, first, to inevitable irregularities of the spark gap; its surface cannot be twice alike, if one speaks very critically; and to the fact that losses and brush discharges take place in the condenser; hence the points at the top
9468 of such a curve are always irregular and located only within a few per cent. That is what I mean when I say, "Within the limits of error".

Therefore these circuits, as shown by the Bureau of Standards report to be in resonance with one another, are subject to that correction. While I am considering this, I might point out that the sheet No. 1, or exhibit K1, also shows the tremendous effect of throwing the circuits out of exact tune. This is illustrated by curves 4 and 5. Noting the position of curve 4, for example, it will be seen that, instead of being tuned to some 658 or

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660 meters, as the two circuits were when curves 1 and 3 were taken, this circuit is resonant to something 750 or 755 meters, perhaps. Now, curve 4 refers to the legend here and the current is said to be two amperes, or the energy proportional to four, since the energy is always proportional to the square of the current.

Looking, however, to the curve 3 when the apparatus is normally adjusted, we see the current is 5.8, the square of which is, roughly, 36—I only speak in general terms—so that the energy in the two cases is as 4 to 36, or, in other words, throwing the circuits out of exact tune, as illustrated by curve 4, has reduced the energy to one- 9470
ninth. You get nine times as much energy by putting the circuits in exact resonance as you do by putting them out that amount from exact resonance and, of course, if they are still further out of resonance the loss is enormously greater.

That shows the effect of putting the two circuits out of exact resonance or out of exact tune, by making the wave length of the antenna or radiating circuit too long.

Now, curve 5 shows the effect in reducing the efficiency, by throwing the two circuits out of exact resonance when the antenna wave length is made too short. In the case of curve 5 the circuit was adjusted to the wave
length as seen from the lower scale, to perhaps 610, as 9471
against the tuned or precise resonant adjustment of about 660. And, referring to No. 5, to the legend on the right, I find that the inscription says that the current then was reduced to three and a quarter amperes, while the square of three and a quarter is, we will say roughly, 14. So that by throwing the two circuits out of exact resonance by making the antenna circuit too short, the energy was reduced two and a half times. In other words, there is two and a half times as much energy given out when the circuits are in exact resonance as

when they are thrown out of exact resonance by the small amount between 600 and 610. A change of wave length, in other words, of about ten per cent, or considerably less than ten per cent, reduces the energy down to only a two and one-half part of what is was before.

Q. Now, Mr. Waterman, you referred a moment ago to the fact that the court might be misled if it considered the height of these curves as representing the efficiency, as I understand it. Now, what is the efficiency when these two circuits are exactly in resonance, as shown in the middle of the figure as distinguished from when they
9473 are not exactly in resonance? A. Well, that is what I have been trying to state. The efficiency when the circuits are exactly in resonance is nine times as great as it is when they are out of exact resonance by having the antenna wave length increased to 755, and two and a half times as great as when they are thrown out of exact resonance by having the wave length reduced only about 40 meters; and I called attention to that in this connection as defining what I mean when I said the tuning was critical. A small change of wave length—a small movement of one of the adjusting points, 1, 2 or 2a, or the condenser on exhibit 68, which only means a relatively small difference in the wave length, makes a very great
9474 difference in the efficiency.

In other words, the apparatus is one which, to get any sort of reasonable efficiency, requires the very closest conformity to the direction of the patent, that the two circuits should be in resonance with one another.

Q. Is it your understanding, Mr. Waterman, from Defendant's Exhibit K10, Bureau of Standards report, or Defendant's Exhibit K1, that these curves are drawn on the same scale? A. No. That was what I was cautioning the court against. These curves are not drawn on the same scale. None of these curves. They do not pretend

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to be. The scale values are given by the legends on the side. It would be very difficult, in making curves of that kind, to undertake to plot them to the same scale and, as I say, if one does not recognize that they are not to the same scale, they are very misleading. For example, curve 4, if plotted to the same scale, would hardly appear above the horizon—it would be very small, if not below the bottom of the sheet, and to give it more exact proportions or practical value, the values have been multiplied by some factors so that it makes the respective height of the curves 4 and 5 appear much greater than they are,
and, therefore, a person has to be careful not to judge 9476
their values from their heights, but to take the values as given in the figures of the legends.

Q. In other words, the mere fact that curve 5 is shown higher than curves 1 or 4, does not mean that curve 5 is more efficient? A. No. On the contrary, the inscription shows it is only one two and a half part as efficient.

(Whereupon an adjournment is taken until tomorrow, Wednesday July 19. 1916, at the hour of 9:30 o'clock A. M.)

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Wednesday, July 19, 1916, 9:30 o'clock A. M.
Continuation of proceedings pursuant to adjournment. All parties present as at former hearing.

F. N. Waterman, same witness, resumes the stand for further direct examination.

Q. (Mr. Betts) Mr. Waterman, will you please refer to sheet 1 of the Bureau of Standards report, Defendant's Exhibit No. 10, produced by Mr. Kolster, and state

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with what degree of accuracy the antenna circuit represented in curve 1 of that figure is in resonance with the spark gap circuit when connected to the antenna as represented in curve 3? A. As I explained yesterday, the top of such a curve as No. 3 is, necessarily, slightly indefinite, on account of the inevitable variations of the apparatus. If I read the curve by the line which the draftsman has drawn, as marked "Curve 3", I find that the top point reads 662 meters. Correspondingly, the top curve 1 reads 658 meters. Being, therefore the same, within four meters or, I presume, a fraction of one per
9479 cent. The dots forming the top are scattered with reference to that curve, more or less irregularly; but if we take the middle position between the irregularly scattered dots at the top, we find that that is also 658 meters. I mention that, not that it is of any importance, but merely as showing that the inevitable error of measurement prevents the reading within two per cent usually—in this case, less than one per cent; but if we take the mean of the scattered points that happen to come exactly——

Q. You mean by "inevitably", such as anyone who is carrying out fairly any test would inevitably make, due to the use of measuring instruments? A. Yes. In other words, no matter what care is used, it is impossible,
9480 owing to unavoidable variations, to have the minimum error smaller than a few per cent.

Q. Mr. Waterman, have you personally adjusted the Simpson mercury valve transmitter which was loaned to the Marconi Company, so as to radiate energy of 600 meters? A. I have.

Q. What did you find as to the exactitude of oscillations or resonance in that transmitter between the antenna circuit and the spark gap circuit when the set was operating to radiate at 600 meters? A. The same result as is shown on sheet 1 for 660 meters. For some reason that does not appear in the report. The adjustment made at the Bureau of Standards was for about 660 meters—658 meters precisely—but the result is the same when adjusted for the legal wave length of 600 meters, within the same small error of measurement. The more precise the exactitude of resonant adjustment, the better the performance; that is, the larger amount of energy radiated.

Q. You referred yesterday, in discussing the diagram of the Simpson mercury valve transmitter, plaintiff's exhibit 68, to the fact that there was a condenser c and an inductance common to the antenna and spark gap 9482
circuits. Did I understand you that both those instrumentalities were what you have called coupling means? A. Both have a coupling effect, yes.

Q. Which of those means, the condenser C or the inductance, is the chief factor in coupling the two circuits together in the Simpson mercury valve transmitter? A. The tests show that the chief factor is the inductance. The connections can be changed by simply shifting the ground lead to the top instead of the bottom of the condenser; in which case the condensor is only in the primary circuit, and this makes almost no difference in the operation of the apparatus. As Mr. Pickard stated, a 9483
large condenser has only a small coupling effect; and this condenser is about seventy times the capacity of the aerial.

Mr. Skeel: A little louder, please, Mr. Waterman.

A. (Continuing) I said, about seventy times the capacity of the aerial; hence its coupling effect is very small. It is interesting to note too that the coupling effect is opposite to that of the inductance. In other words, having it in the aerial common to the two circuits does not tighten the coupling, but loosens it; for the reason that what we call the capacity reactance is opposite in phase to the inductance reactance; hence the two subtract, and the inclusion of the condenser in the aerial has the effect, therefore, not of tightening the coupling, but of slightly loosening it. Therefore, the inductance is the determinative and dominating factor in the coupling.

Q. Will you please now compare the mode of operation of the transmitter of the Marconi patent 763772 with the mode of operation of the Simpson mercury valve transmitter, and state whether or not in your judgment
9485 there is any essential difference between the modes of operation of the two transmitters; giving your reasons for any opinion which you may express? A. My study of the Simpson transmitter and of the Bureau of Standards report as to its operation, indicates that there is no difference whatever.

The Court: No difference in the operation?

A. (Continuing) No difference in the operation. The Marconi patent does not set forth any theory of operation. That is, it does not go into a theoretical discussion. It describes the circuits, sets forth the primary
9486 circuit which is in it, a condenser, an inductance, a spark producer and an aerial circuit, which is the elevated capacity, the elevated wire connecting to it, and inductance interposed in the connection to earth. It shows a means of associating the two circuits so that energy may be transferred.

The specification, as I have said, does not indulge in any theoretical speculations as to what it is in detail that is going on within the circuit; but merely makes it clear that if there is a condenser in a primary circuit which is arranged to be charged by a source of energy, as shown in Fig. 1 of the patent, and provided with a trigger de-

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vice or oscillation producer; then upon the upsetting of the equilibrium of the energy originally statically or quiescently stored, energy will be transferred to the aerial circuit which, by virtue of its oscillations, will send out the electrical waves.

In precisely the same manner, in the Simpson mercury valve transmitter (referring now to exhibit 68), the condenser c is charged and the equilibrium of the circuit is upset by the oscillation producer g, and is thereby set in oscillation and, through the inductance dd^1 in this apparatus, just as in Fig. 1 of the patent in suit, energy is transferred in an oscillating form to the antenna and is 9488
sent out in waves.

The characteristic of five out of the six circuits described in the Marconi patent is the large ratio of capacity to inductance, to which I have already referred. This has certain marked effects upon the specific or precise operation, in that it causes energy to be discharged through the trigger circuit in the figure of the patent and in the Simpson mercury valve transmitter, as a large current. The circuit of the patent is, in other words, a circuit of high natural decrement, which means that the energy is transferred quickly from one circuit to the other. 9489

There is a very excellent illustration of the effect of this ratio of c to L in an oscillatory circuit, given in the book which Mr. Pickard has referred to so often, namely, "Fleming's Principles of Electrical Wave Telegraphy."

Q. What edition, Mr. Waterman? A. I am referring to the 2nd Edition, which is the one Mr. Pickard uses, I believe, and particularly to the plate which is inserted opposite page 111 (showing plate to the court). The first five of these diagrams, that is to say the entire top row, and the first one in the lower row, are made with a cir-

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cuit of constant resistance and constant inductance, but of variable capacity.

Q. That is a larger and smaller amount of condenser? A. A larger and smaller amount of condenser.

The first one is taken with the largest amount of condenser; the next one is a little less, and the next is a little less; and it will be noted that the first one, which has this large ratio, shows two, and a small fraction oscillations; the next one shows, approximately, three, and the next one three and a fraction, and so on; the number increasing until in the 5th there are seven.

9491 So this large ratio of capacity to inductance has the effect of producing what is called a highly damped circuit—circuit of large decrement.

I may say that these curves which I have just referred to in the Fleming book are taken with a circuit of relatively slow period. That is to say, it has been impossible, so far as I know, until Dr. Chaffee succeeded in doing it, to actually get diagrams, on account of the very high frequency used in commercial wireless telegraphy, particularly ship frequencies, and these diagrams which I have just shown, are made with the so-called Dudel oscillograph and the exact height preserved, and the proper ratio of c to L. They have both of these
9492 quantities increased so as to bring the frequency down. For example, the frequency in Fig. 1, which I just referred to, is only 339 per second, while the frequency of the normal ship's wave length is five hundred thousand.

Q. Per second? A. Per second. But these are taken for the purpose of illustrating the effect of that ratio of c over L; and the circuit having the large ratio c over L has the small number of oscillations, necessarily. Hence the mode of operation of the apparatus described in the Marconi patent, with particular reference to certain of the specific illustrations which I have been referring to

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heretofore, is one in which the energy in the closed oscillatory circuit is rapidly dissipated; that is, rapidly transferred to the aerial, in so far as it is transferred and consumed in the resistance—of course to a certain extent always, because the gap and the conductors, both, necessarily have some resistance.

To a similar extent the statements are true as to plaintiff's Exhibit 68, that is the Simpson mercury valve transmitter. It has the high ratio of capacity to inductance, and in its trigger and primary oscillatory circuit, and hence the energy is transferred quickly; that is, there
are only a few oscillations. In this respect, also, there- 9494
fore, the Simpson mercury valve transmitter is such a transmitter as is shown in the Marconi patent in suit; that is to say, it has the mode of operation there described.

This fact is stated by Mr. Kolster specifically with respect to the Simpson mercury valve transmitter.

Dr. Kolster says, on page 1124 (Print p. 1222, Vol. 3) of his testimony of the record:

> "One of the characteristics of this circuit" (referring to the Simpson mercury valve primary circuit) "is the fact that this capacity, the antenna capacity c"——

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Mr. Kolster calls it "antenna capacity" because it happens to be in the antenna also——

> "Or the capacity in the antenna circuit c, is large, and that part or portion of the inductance w, across which the gap s is placed"——

In the diagram which he was using this, which is marked dd was called w——

> "is small, and it has that characteristic of having a large ratio of c over L of the ratio of capacity to

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inductance. That is a mathematical ratio of some significance and of important significance in the operation of radio apparatus''.

Then on the next page he says that this has the effect of causing the oscillations to be very few—just as I have shown by the Fleming book.

So that there is no dispute about the matter that this use of the minimum inductance in the primary circuit, and consequently, for the given wave length, of the maximum capacity, giving the large ratio of c over L, has the effect of forcing the transfer of energy to take place in a
9497 very few oscillations.

In this respect, therefore, the Simpson mercury valve transmitter is identical with that described in the Marconi patent.

The Court: Let me ask what page is that of Kolster's testimony.

A. (Continuing.) Pages 1124-5, (Print pp. 1222-3, Vol. 3) your honor,—I referred to both.

The Marconi patent shows an antenna circuit having a capacity area f at the top, and the vertical wire—such a vertical circuit with capacity area at the top, is a good
9498 radiator of energy. In order that the energy radiated may not, however, be radiated too rapidly, Marconi places in the elevated antenna circuit an inductance marked g in Fig. 1. May I add the letter g to this?

Q. Yes, please do so. A. (Continuing) I have added the letter g to plaintiff's exhibit No. 1. The g is an inductance coil placed in the antenna circuit, and its effect is, as has already been pointed out in connection with the Lodge patent, in preventing the radiation from being too rapid, that is, in giving more sustained radiating waves. This effect of giving more sustained radiating waves is secured in the Simpson mercury valve transmitter by the insertion of the coil g, on exhibit 68 in the same manner and for the same purpose. It is in the Marconi patent also used as an adjusting or tuning means, and it is so used also in the Simpson mercury valve transmitter. The two are, therefore, alike in this respect, They are further alike in the respect that the antenna circuit is the radiating circuit and the closed oscillatory circuit is not a radiating circuit, as is clearly set forth in the patent, and it is agreed to be the fact with reference to the Simpson mercury valve transmitter.

Mr. Simpson says, at page 1057 (Print p. 1164, Vol. 2) 9500
of the record, beginning with the question at line 12:

> "Q. Now, does the antenna circuit of the Simpson transmitter radiate energy? A. The antenna circuit? Yes, certainly.
>
> "Q. Does it send out all energy which can be received at a distant station? A. Energy which can be received at a distant station—I do not understand the question.
>
> "Q. Does the antenna of the Simpson transmitter send out energy which can be received at a distant station? A. It certainly does".

Mr. Simpson also agrees that the closed oscillatory
or trigger circuit, like the closed oscillatory circuit trig- 9501
ger circuit of the Marconi patent, Fig. 1, does not radiate energy. On the same page, 1057, line 22, he says—reading question and answer:

> "Q. Does the circuit which you have called the converting trigger circuit of itself radiate or send out energy which can be received at a distant station? A. I have not called it a converting trigger circuit. I have called it a converting trigger, and it does not send out energy which can be received at a distant station."

9502 F. N. Waterman—Recalled—Direct.

Hence it is agreed that the two circuits of the Simpson mercury valve transmitter have the same operative functions in this regard: The one has the purpose of sending out the energy; the other has merely the purpose of receiving the energy and setting it into a state of oscillation.

I call attention to the fact that the quotation which I just read shows that Mr. Simpson refuses to recognize the circuit consisting of the condenser, two inductances and the oscillation producing means, or spark gap, as a trigger circuit. He prefers to leave off the word "cir-
9503 cuit". In the Bureau of Standards report, however, your honor will find some ten or a dozen references to it as a circuit, and, as I have stated, there is no reason why it should not be called a circuit; but the reason Mr. Simpson gives for calling it merely a trigger, is that portions of the two circuits are in the primary circuit and in the antenna circuit, but it was agreed—which Mr. Simpson evidently forgot—by Mr. Pickard that it is entirely immaterial whether we got our coupling, our union by two coils distinct from one another but closely associated, or whether by a member, either such as the capacity at Fig. 1 and 2 of G. W. P. 4 or inductances like Fig. 3 of G. W. P. 4 (Vol. 2, p. 456), which is common to the two
9504 circuits.

I read yesterday Mr. Pickard's statement that the operation was precisely the same; and that fact is recognized in the government report by the universal designation of the primary circuit by its proper term "circuit".

I find, therefore, that the mode of operation of the Simpson mercury valve transmitter is precisely that of the transmitter set forth in the Marconi patent. That is illustrated both in the proper operation and in the improper operation,—of course it is possible to design any device badly or design it well, and within limits it is possible to operate it badly or operate it well.

Now, as has been explained, if the device of the Marconi patent is operated badly or designed badly; that is to say, if the coupling is made too close for the quality of the spark gap, there will be inferior operation—it is the same sort of operation, but it is inferior, in that there will be a waste of energy in the primary circuit, due to the fact that after some of it has gotten into the antenna—after it has all gotten into the antenna, I should say—instead of its all being radiated, some of it is transferred back. That means that this spark gap which, as I 9506
pointed out yesterday, is a part of two circuits, has not ceased to operate when the energy has been transferred but has been, by the operation of the other circuit or some other way, kept in operation. Now, the government report shows that exactly the same thing is true of the Simpson mercury valve transmitter

Q. You are now referring to defendant's exhibit No. 10? A. I am referring now to defendant's exhibit No. 10, and particularly to sheet No. 3.

In any such transmitter we get the best spark gap operation which is possible, and then make the coupling as close as that spark gap will permit. 9507

In getting this curve of sheet No. 3, Mr. Kolster introduced an inferior spark gap, the plain open gap. He did not alter the coupling. And the result with the Simpson mercury valve transmitter was exactly the result that always happens with a transmitter of the Marconi patent when the same thing takes place, or is done, I should say; that is to say, Mr. Kolster's sheet No. 3, showing the operation of the Simpson Mercury valve transmitter, is the most conclusive possible proof of the identity of the apparatus of that transmitter with the apparatus shown in the Marconi patent Fig. 1. Give

9508

either one of them inferior operation of the spark gap and we will have this peculiar irregular type of resonance curve.

But this sheet No. 3 of the Bureau of Standards report shows another very interesting thing, and explains it fully. That is to say, the text accompanying it explains it fully.

This curve of the Simpson mercury valve transmitter shown on sheet No. 3 of exhibit No. 10 is seen to be not merely a curve with a little irregularity at the left and a slighter irregularity at the right, indicating
9509 the presence of the so-called coupling waves. The tendency of the circuit to beat, as it has been explained fully —I believe your honor will remember that expression— it merely means that a little of the energy is transferred back, instead of being radiated outward. The report shows that, notwithstanding the fact that there is a little transfer of energy back, the main curve rises very high and very sharply, and the report explains that notwithstanding the small evidences of beats here,—which evidence is conclusive as I have said, as to the fact that the Simpson mercury valve transmitter has identically the same sort of association of its two circuits as given in the Marconi patent—this very sharp upper portion of the
9510 curve shows the particular advantage which the Simpson mercury valve transmitter derives from the fact that it is not merely the circuits of the Marconi Company, but the particular relations of constants, that is the high ratio of capacity to inductance, and this is explained in the report with reference to this sheet 3.

I will read the entire statement regarding 3, which is found on page 5, beginning on line 19 of the report:

> "On sheet No. 3 are given the results obtained when the special type of spark gap was replaced by an ordinary plain, open gap and without the

 95[11]

> use of the mercury valve rectifier. The resonance curve indicates the existence in the system of two wave lengths in addition to the free or natural wave lengths of the antenna circuit. This behavior is characteristic of a system with two degrees of freedom where no attempt is made to suppress the coupling waves''.

That statement ''no attempt is made to suppress the coupling waves'', means no attempt is made to get proper operation of the spark gap.

> ''In this particular case the free or natural wave length of the antenna circuit appears more 95[12]
> prominent than is usual. This is largely due to the fact that even without the use of a special type of spark gap, the oscillations in the trigger circuit are comparatively highly damped because of the small value of L over c''.

That is to say. of course the larger value of c over L—sometimes you put it one way and sometimes the other.

In other words, this advantage which the Simpson mercury valve transmitter gets by virtue of its following of the Marconi patent, is the existence of a sharp wave, notwithstanding the fact that there is some re-
transfer of energy and this, as the report shows, is due 95[13]
to the large ratio of c over L, or as you might say, the small ratio of L over c, which the Simpson mercury valve has by virtue of its very literal following of the disclosures of the Marconi patent.

I note that in the statement which I just read the report refers to the fact that sheet 3 was taken not only with the plain gap used, but also with the Simpson mercury valve removed, and it might be questioned whether the removal of the mercury valve had something to do with the operation.

9514 F. N. Waterman—Recalled—Direct.

If the Court will observe sheet No. 5.

Mr. Betts: Of the same report?

A. (Continuing) Of the same report—you will find the same apparatus—the Simpson mercury valve apparatus operated in its normal way, save for the omission of the mercury valve. and a reading of the report and a study of the curves shows that this mercury valve has nothing whatever to do with the operation. This report makes that very clearly and very conclusively evident by actual numerical results.

9515 Not to go into this matter at length at the present time. I will call attention to the fact that on page 7 there is given a result entitled "Number of Oscillations based on Taege's Work, 2.1".

Q. That is at what line, Mr. Waterman? A. That is the very last line on page 7.

While, with reference to sheet 5, which is identical save for the omission of the mercury valve, we have also in the very last line of page 8, "The number of oscillations, based on Taege's work, 2.2".

Those figures might have been reversed, obviously, without any significance whatever. In other words, the
9516 circuits are alike, within the necessary uncertainty of such measurements.

So that the Bureau of Standards report shows, as the Cambridge, or Cruft laboratory test shows, that the mercury valve had absolutely no effect on the operation whatever.

I concluded, therefore, that the Simpson mercury valve transmitter is in all respects such a transmitter and has such a mode of operation as is set forth in the Marconi patent.

Q. Please state what. if anything, you had to do with the making of any tests on defendant's Simpson mercury

 9517

valve transmitter, since the suspension of this case in April, 1916? A. Since the date mentioned I have,—well, I can best answer that question by simply stating very briefly the sequence of events.

On the 17th or 18th of May I was in Connecticut, I received a telegram from Mr. Weagant of the Marconi Company asking me to go to Boston and interview Dr. Chaffee of the Cruft High Tension Laboratory of Harvard University, and ascertain what equipment he had and what time he could give to the matter of making tests.

I might say that in the interval between April 30th 9518
and that date, about May 17th, Mr. Weagant and I had canvassed, so far as such information as we could get hold of, the laboratories of the country, and we found that there was only one laboratory which was equipped, as far as we could ascertain the facts, to make such tests. We found also that there was apparently only one man in the United States who was capable of making the tests. We subsequently found, however, that it, perhaps, might have been made at Columbia University by Professor Morecroft, because, unknown to us, he had also spent a number of years in working with the Braun tube. But I went to Cambridge and interviewed Dr. Chaffee; told him
the Simpson mercury valve transmitter was involved in 9519
litigation; that the Marconi Company desired to find out what the actual facts were as to the operation of the transmitter, and that if he had the time and the equipment for the purpose, the apparatus would be sent to him for tests. I discussed——

Mr. Skeel: Your honor, I object to this as hearsay. It is all hearsay and for the obvious purpose of trying to inflate the value of Professor Chaffee as a witness. I think the question is, what was done at the time of the tests. I do not

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think these ex parte conversations between Mr. Waterman and Professor Chaffee are material, and I do not think that Mr. Waterman's opinion of Professor Chaffee is material.

Mr. Betts: If the court please, your honor will remember the cross-examination of Dr. Chaffee yesterday, which you read during the recess, and there was a good deal said in the cross-examination as to what Mr. Waterman had to do with those tests, and whether or not they were conducted by Mr. Waterman or at his suggestion and
9521 so forth and so on. I think the whole story should be told.

Mr. Skeel: Let him state the facts.

The Court: The objection is sustained as to the conversation; Mr. Waterman may state what he did.

Mr. Betts: Yes.

The Court: Not what was said, though.

A. (Continuing) What I did was to briefly explain the bare facts as to what the transmitter consisted of and how the several elements were connected, to Dr. Chaffee, to look over the equipment which he had for the
9522 purpose of making the tests, and ascertain from him what dates he had open for the tests, and my recollection is that subsequent events complied with the program which he then laid down.

I then returned and reported the facts ascertained to Mr. Betts, and upon his instruction, ordered the apparatus sent, that is to say, I merely acted as a messenger to report to Mr. Weagant the instructions which Mr. Betts gave me.

Q. What generator did you order sent with the Simpson mercury valve transmitter to Dr. Chaffee of the

Cruft High Tension Laboratory? A. I merely requested that a 500 cycle generator would be sent.

Q. Why? A. Well, at your instruction; that is, at Mr. Betts' instruction—there was a discussion between us as to the matter, and you told me that your decision was——

> Mr. Skeel: I object to this as hearsay; if Mr. Betts wants to give the reasons, he can be sworn. He asked Mr. Weagant yesterday why he did it and Mr. Weagant said because Mr. Waterman instructed him, and now Mr. Waterman says that 9524
> Mr. Betts instructed him—I am willing that you shall testify.

Q. (Mr. Betts) You got the instructions from me and you carried them out? A. Yes.

Q. To send a 500 cycle generator? A. Yes.

Q. Are you able to state what the reason was? A. I am.

Q. What was it? A. A study of the Kolster report showed that the facts as to the operation with the 120 cycle generator were quite fully set forth in that report, and a study of the record showed that the tests at the Washington University were made with the 500 cycle gen- 9525
erator. You told me that you considered it unnecessary, therefore, to repeat the tests with the 120 cycle generator, but that you thought it important that the conditions of the Washington University tests should be duplicated in that respect. Therefore, you instructed me to send a 500 cycle generator.

Q. Do you remember what generator was used in operating the Simpson mercury valve transmitter before the court at the Kilbourne & Clark laboratory here in Seattle, whether it was a 500 or a 120 cycle? A. I do. It was a 500 cycle machine.

9526 F. N. Waterman—Recalled—Direct.

Q. Were any tests made in your presence on the defendant's Simpson mercury valve transmitter as the result of what you have said regarding the shipment of this transmitter to the Cruft High Tension Laboratory? A. There were.

Q. When were those tests conducted in your presence? A. I was present when tests were made on June 20 and 21 and July 1, 2, 3 and 4.

Q. Who conducted those tests on July 3 and 4? A. Dr. Chaffee—Dr. Chaffee conducted all the tests.

Q. What, if anything, did you have to do by way of
9527 direction or supervision or suggestion as regards the tests or the methods of tests conducted on July 3 and 4? A. Nothing whatever. Dr. Chaffee occasionally asked me questions, which I answered, but I had nothing whatever to do with the tests.

Q. Did you attend or see all of Dr. Chaffee's tests on July 3 and 4, at the Cruft High Tension Laboratory at Cambridge, Massachusetts? A. I did.

Q. And what motor generator was used during those tests? A. A Crocker-Wheeler 500 cycle motor generator.

Q. And it had a name plate on of what concern? A. It had the Marconi name plate on it.

9528 Q. I will ask you first as to whether anybody representing the defendant was present at the tests on July 3 and 4, at the Cruft High Tension Laboratory? A. There were several people.

Q. Can you give any of the names? A. There were present, representing the defendants as I understood, the following gentlemen: Mr. Farnsworth, Mr. Pickard, Professor J. F. Stone, Mr. Simon, Dr. Zenneck, Mr. Proctor, Mr. Barkley and Mr. Kolster; also Mr. Simpson.

Q. Will you please now describe the methods of the tests made by Dr. Chaffee on July 3 and 4, as you saw them in the presence of defendant's representatives; of course I am now referring to the tests of the Simpson mercury valve transmitter? A. May I have the diagram of connections—I can do it, most readily with the diagram of connections.

Q. Dr. Chaffee's diagram? A. Yes.

Q. (Handing document to witness) Please refer in that connection, if you please, to "Chaffee Diagram No. 1." A. Dr. Chaffee made a number of different sorts of tests with the Braun tube. The Simpson apparatus—the Simpson mercury valve transmitter apparatus was set up in Dr. Chaffee's laboratory at the Cruft High 9530
Tension Laboratory building, just as the court saw it set up in the Kilbourne & Clark laboratory in the spring. An artificial or dummy antenna was employed, such as the court also saw employed at the Kilbourne & Clark laboratory in the spring, and such as Mr. Simpson described as being used in the Washington University tests, and such as set forth in the Bureau of Standards report, as used in that test.

Dr. Chaffee had, in my presence, adjusted the Simpson mercury valve transmitter to give the best results that could be obtained with it, and the tests were conducted with the apparatus in that condition. The Braun tube, which is illustrated at the left of Chaffee diagram 9531
No. 1, was employed.

This tube consists of a glass cylinder having at its lower end a fluorescent screen b mounted in it,—it is to be seen at the extreme left of the diagram. This has leading from it a tube, which at the other end has a cathode k sealed into it. An anode is also sealed into the glass at a and below the anode in a diaphragm d, also sealed within the tube. Still further down there are two small plates p parallel to and opposing one another; also sealed within the tube.

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For the purpose of the first tests, Dr. Chaffee made use only of current deflecting coils, and Dr. Chaffee testifies that this was because such coils were the only suitable means in his opinion, of causing deflection of the cathode beam.

These coils are shown in section at E, there being four such letters on the diagram. These letters indicating the sectional end view of a pair of coils, each consisting of one turn only, placed on opposite sides of the tube.

Between those coils and the glass of the tube, was a screen, which I think has no letter of reference in the
9533 drawing, but is seen inside the letter e, and outside of the plates P. This drawing Chaffee describes in detail in his deposition. Its function was to prevent there being any electrostatic effect, due to the coils e, and insuring that only the magnetic effect of those coils should be experienced by the cathode beam.

To excite the tube there was used a high tension battery, which is seen diagrammatically indicated at the top of the diagram just above the Braun tube.

This high tension battery had its negative terminal connected by a wire leading from an adjacent building over to the Cruft laboratory on extemporized insulation, and led to the cathode of the tube.

9534 The other terminal was connected to earth, and the anode of the tube was similarly connected to earth. A switch was provided, I should have said, in the cathode connection, so that the tube could be connected or disconnected at will.

Also, as is noted on the diagram, the diaphragm d was earthed.

Outside of the cathode tube, or Braun tube, was a coil f, which could be excited by a constant current, that is a direct current from the battery 40, through a variable resistance r.

 9535

This was a sort of focusing device, if I may use that term, that is a device for making as small and concentrated a spot of light as possible.

The mode of operation of the tube may be briefly stated as follows: Upon the closing of the switch the current from the high tension battery, which was, as I remember, in Dr. Chaffee's tests something of the order of thirty-five thousand volts at the start, but which fell off very rapidly as the day went on, owing to leakage of the insulation and consequently the depletion of the battery—that is the leakage of the line insulation—the flow
of this high tension current within the tube caused a 9536
stream of cathode rays; that is to say, of electrons or corpsules, to be emitted from the cathode k.

Q. The switch you speak of is at the top of the Braun tube, is it, connecting with the battery? A. Yes, at the top of the Braun tube.

This stream, passing through the diaphragm d and impinging upon the screen b at the bottom of the tube, the bombardment of this screen by the high velocity electrons or cathode rays, caused a fluorescent spot, or spot of light, to appear on this fluorescent screen, due, of course, to the fluorescence of the screen material.

This spot was concentrated, that is made smaller and
more sharply defined, by the use of the coil f, but when 9537
once set for a given test, was not altered, of course.

This stream of electrons is made up of negative electrical charges; that is, the electron is a negative charge of electricity which behaves as though it had a mass of only one-eighteen hundredths part of the mass of the smallest atom known, namely an atom of hydrogen.

Such a massless beam, that is a beam of particles of such very small inertia, can be deflected very rapidly, and the purpose of the coils e, placed on each side of the tube, was to deflect the beam.

9538 F. N. Waterman—Recalled—Direct.

These coils were connected by wires to a little bit of wire in the Simpson mercury valve transmitter.

That part is marked s^1 and s^2, and I will designate it here on Exhibit 68 (page 3133) as the two points which I am now pointing out.

Q. Please apply the same letters to Plaintiff's Exhibit No. 68? A. In reality it is a little lead wire about two inches long which is found in the transmitter leading from the low potential end of the spark-gap to the low potential end of the condenser. It is a bit of braided wire, as I remember it, which is perhaps two inches
9539 long. Dr. Chaffee, in my presence, made certain calculations as to the characteristic properties of the circuit which he could connect around this little lead without in any way disturbing or in any appreciable way disturbing the operation of the Simpson mercury valve transmitter, and which should faithfully shunt a constant portion of the primary current, and having made his calculations—this he did prior to July 3rd, but in my presence—he made tests to determine a suitable circuit. This pair of coils e e, therefore, simply shunted a portion of the current. I have not Dr. Chaffee's figures, but my recollection is that the current shunted was about a quarter of the total current of the primary circuit. This
9540 current flowed through the coils e e, so that it always bore a constant relation to the current of the primary circuit, and hence, the spot of light or cathode rays were affected by the current of the primary circuit of the transmitter in a strictly proportional manner. The influence of the coils, as Dr. Chaffee showed to me—although I am not sure he did it on July 3rd—is to cause a deflection of the beam, and he did this by applying a direct current from a battery to the coil and noting the deflection of the spot, and he showed that if the polarity was reversed, then the motion of the spot reverses, and

when he varied the current then the motion, extent of
deflection of the spot, was varied. In other words, Dr.
Chaffee completely checked up the operation of this coil
by applying a definite direct current. When this coil
was used the effect of the operation of the transmitter
was to cause a deflection of the spot on both sides of its
zero point, and a deflection which was nearly, but not
quite, equal in extent on both sides of the position of
rest. This substantial equality, or approximation to
equality, of deflection in opposite directions showed not
only that there were oscillations, but that there were
several oscillations in that primary circuit. 9542

When the apparatus, that is, the Simpson mercury
valve transmitter, could be persuaded by careful adjust-
ment of the power supply to operate with great regu-
larity and give a good note, then there were clearly seen
upon the screen spots of light upon each side of the
central position, those spots showing the points at which
the beam of cathode rays rested a moment as it turned
in its back and forth course, and there were usually
visible to me three on one side and two on the other.
Sometimes I could see only two on each side, and some-
times I could see three on each side. Usually I could see
three on one side and two on the other. There were
times when the apparatus was operating irregularly 9543
when all one could see was the straight lines in opposite
directions, and it was not possible to distinguish the
spots, the reason for this being that the spark-gap was
breaking down at different voltages, consequently, the
initial excursion of the spot varied greatly, hence, the
spots of some oscillations were in different positions
from the spots of others, due to other discharges of the
gap, so that the effect was to produce no localized spots,
as would have been the case if the operation was per-
fectly regular, but a line on each side, due to lack of
coincident repetitions of the phenomena.

9544 F. N. Waterman—Recalled—Direct.

The phenomena which were being investigated took place at a terrific rate, namely, at a time of only two one millionths of a second for each oscillation, one millionth of a second for each half oscillation, so that the time when the cathode beam paused at the end of its deflection was very brief for a single oscillation, and apparently not sufficient to excite the fluorescent state in the screen. It required, apparently, a repetition many, many times to give a sufficient impression upon the screen so that the eye could recognize it. At any rate, it evidently required many exact repetitions of the phenomena to enable one
9545 to see it, and it required enormously more of these to enable a photograph plate to show any record of the events. The photographic plate, in other words, is so extremely insensitive, compared to the velocity of the phenomena going on, that, if I remember rightly, Dr. Chaffee said—I think the average figure which he gave was something in the neighborhood of ten or twelve thousand repetitions. Of course it requires a tremendously and unbelievably exact precision of operation in apparatus to repeat itself that number of times without variation.

The next method of test which Dr. Chaffee used retained all of the connections which I have just described,
9546 that is, retained the coils e e, connected as described, but made use of a second excitation, which was, as we say, a function of time. That is, he introduced the time axis. Referring for the moment back to the Fleming book, and the plate opposite page 111; the court will observe that where the motion reverses there is a little spot of light. Now, if there were no drawing out sidewise of the phenomena we would have these spots in one straight line. That was what Dr. Chaffee first did. In his second test he applied a means of simultaneously showing the effect of time, that is, spreading out sidewise the oscil-

lations, thereby getting on the screen an effect which was sometimes like Fig. 1 that I referred to, and sometimes like Fig. 2, and more often as I observed it intermediate between.

Mr. Hughes: Figures 1 and 2 being from what?

A. Figures 1 and 2 of plate 1 of this Fleming book, 1910, or 2nd Edition, that plate being found opposite page 111.

The method of getting this time axis is illustrated in Chaffee diagram No. 1. It consisted in using the inside
plates as a condenser, if I may use that expression, that 9548
is, charging these plates, and then so constituting the circuit that the discharge was not oscillatory, but was a slow discharge with time, the total time of discharge occupying the greater period of time than the oscillations of the primary circuit. When this additional means of deflecting the beam of cathode rays was employed, what happened was that upon the charging of the condenser of the Simpson mercury valve transmitter the spot of light moved from its normal position a certain distance, and upon the discharging of the condenser that spot returned to its normal position, but while it was in the act
of returning it was deflected sidewise by the coils, and, 9549
therefore, it traced the outline of a damped oscillation train. The oscillation train which I saw upon the screen consisted usually of two and one half oscillations. Sometimes only two, sometimes three. Roughly, these Figures 1 and 2 of Plate 1 of the Fleming book, opposite page 111, to which I have just referred, represent what was seen on the screen, with this difference; that the time when the oscillations were taking place was so very, very short, as compared to the time between oscillations—the sparks, for instance, occurred at intervals of one five hundredth of a second, while the oscillation occupied

only two millionths of a second for each oscillation—and as there were two to three or four or five, whatever the fact may be, some small number of oscillations in that circuit the central course of the beam of light appears on the screen traced across the diagram. On photographic plates this effect was very marked, due to the halation of the plate and to the scattering of light. This is a necessary defect of the Braun tube oscillograph, which cannot be entirely overcome.

By these two methods Dr. Chaffee directly investigated what was happening in the primary circuit of the
9551 Simpson transmitter, and my observation in the six days that I watched it, altogether, showed that there were always oscillations in that circuit.

The next method which Dr. Chaffee used was an indirect method, but a peculiarly clear and convincing one. It has been explained that as the primary circuit oscillalates there is a building up of energy in the antenna circuit, and after the primary circuit has transferred its energy to the secondary circuit, whatever energy remains in the antenna circuit is then radiated as a continuation of the process. If, therefore, there was an oscillatory transfer of energy, or a transfer of energy by an oscillating current to the antenna circuit, this
9552 building up of current in the antenna circuit would give evidence of it. Therefore, Dr. Chaffee connected in his third tests the coils e e of the Braun tube around a portion of the lead wire of the antenna circuit.

Q. How is that lead wire indicated in Chaffee diagram No. 1? A. The points of connection are indicated at x x.

There was seen upon the screen oscillations in the antenna circuit building up in value from zero to a maximum through about two and one-half to three oscillations, thus showing that the transfer of energy from

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the Simpson mercury valve transmitter primary circuit to the antenna circuit was by virtue of the oscillation of energy in the primary circuit. In other words, that the energy was stored in the condenser c of the Chaffee diagram No. 1, and also of Exhibit No. 68—discharged across the oscillation producing means, or spark-gap G—transferred by virtue of oscillations to the antenna circuit, building up the antenna current at each oscillation, in other words—a certain building up of current at each half oscillation, a certain building up of current in the antenna circuit occurred. During the next
half oscillation the current so built up was maintained 9554
and increased, so that it became still higher. During the next half oscillation it was maintained and further built up. During the next half oscillation, maintained and still further built up, so that the current approached a maximum value.

Another interesting thing about this method of test is that in the first set of tests, that is, the tests where the connection was to the primary circuit, what we saw with our eyes was the first portions of the train, the last portions of the train being obscured by the blur and haze due to the central spot and its motion. What we saw in this other method of test, where the connection was
to the antenna, was the last, the effect of the last oscilla- 9555
tions, the effect of the first being in its turn more or less obscured by the blur of the central spot. In other words, of course the oscillations start with no energy at all in the antenna and large energy in the local circuit. As the local circuit oscillates its oscillations decrease in amplitude, because it is imparting its energy to the antenna. They, therefore, start large and die down, as I have pointed out. In the antenna circuit, where the energy is being accumulated, they start small and build up, so that we see in the antenna circuit demonstrations

of the effect of the latter part of the primary train distinctly, whereas in the first two methods of tests we see distinctly only the first two or three oscillations.

These were the tests that Dr. Chaffee made to reveal what was happening in the Simpson mercury valve transmitter when in normal operation.

He made another group of tests later, that is, after the completion of these.

Q. In your presence? A. In my presence.

Q. On the dates above mentioned, July 3 and 4? A. Yes. This group of tests comprised three different tests,
9557 as I remember, in which the apparatus was not in its precise normal condition.

First, he made tests in which he cut out the mercury valve shown at z on exhibit 68, that is to say, he did not start the keep-alive circuit, but, on the contrary, had his switch open, and he short-circuited the value, as is described in the Bureau of Standards report, the same method that Mr. Kolster used in his tests of the Bureau of Standards report, shown in sheet No. 5. This series of tests showed that the operation of the primary circuit of the Simpson mercury valve transmitter was not affected in the slightest degree, the circuit was equally oscillatory. In that case the straight line deflection ex-
9558 tended equal distances each side of the spot.

The next test consisted in transferring the ground connection, that is, the whole Mercury valve transmitter was operated in its strictly normal way, the connection to earth, however, being transferred to the top of the condenser. Of course, Your Honor understands a dummy——

Q. (Interrupting) You are now referring to? A. To the top of the condenser c.

Q. In plaintiff's exhibit No. 68? A. Yes. A dummy antenna was actually used, and the change made corre-

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sponded to what I now indicate, that is, cutting off the lead at the point s2, and transferring it around to a point above the condenser, below the inductance dd[1], or below the point 2. The coupling then was only the inductive coupling due to dd[1] on exhibit 68, and the condenser was only in the primary circuit. It was not a part of the antenna circuit at all, and the effect seen on the screen was precisely the same as in the first instance, when the connections were precisely as shown in 68, the normal Simpson mercury valve connections.

This corresponds precisely with what Dr. Kolster
shows in his Bureau of Standards report—no, I beg par- 9560
don, Dr. Kolster did not make that test. It operates as an oscillatory circuit and oscillates just as much one way as it does the other. That is, the Mercury Valve does not affect the oscillatory or non-oscillatory character of the primary circuit. It is a circuit whose oscillatory character is determined by this ratio, C over L and its coupling to the antenna circuit.

Dr. Chaffee also made another test in this same series, namely, the test which corresponds to sheet No. 2 of the Bureau of Standards report. That is to say, he disconnected the antenna entirely and tested merely the primary circuit, and the result was to show that the primary
circuit oscillated, and there were visible upon the screen 9561
to my eye more oscillations in this case than in the other. This, of course, must be the case, because in the event that the antenna is not present there is nothing to which the primary circuit can transfer its energy, hence, the same amount of energy must take longer in being consumed in the resistance of the primary circuit, and I could see on the screen four oscillations in this condition, four complete oscillations with an indication that there were more. One of the photographs which Dr. Chaffee took shows seven or eight, if I remember correctly.

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Dr. Chaffee then made another distinct series of tests, in which the connections of the Simpson mercury valve transmitter were normal.

Q. That is, with the mercury valve in circuit and with the condenser in circuit, as shown in plaintiff's exhibit No. 68? A. Yes. But he used outside electrostatic deflecting plates. The way it happened that he did that was this; I stated to him that outside plates were used at the Washington University tests, and he said that reliable results, or even interpretable results could not be obtained, as far as he knew, with outside plates, and
9563 that he would make tests to ascertain the fact. He made such tests in my presence. I think it was on the first day of July, and these tests he repeated on July 3rd and 4th in my presence. May I have the photograph that was put in evidence, defendant's exhibit F. G. S.—4. (Defendant's exhibit F. G. S.—4 placed on easel.) Dr. Chaffee showed that tests made with outside plates, that is, plates outside of the tube, were entirely unreliable and uninterpretable. He showed, for example, that the position of the spot was the same whether the condenser was charged or not charged, and that what the spot moved in response to was change of charge, so that it would be substantially impossible to interpret the result.

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Mr. Hughes: You say, "he showed", you mean that his tests made on July 3rd and 4th showed what you have stated.

A. Yes, I mean what I saw on the screen as the result of what Dr. Chaffee did showed that the results were entirely unreliable and uninterpretable.

In F. G. S.—4 (Vol. 2, p. 1102), which Mr. Simpson produced, Mr. Simpson stated that in diagram No. 1 of that photograph the deflection of the spot to one side only showed that there were not oscillations in that primary circuit. Dr. Chaffee obtained with the set operating in its normal connections, this photograph like No. 1, and he also obtained with the same connections and the same circumstances a photograph like No. 2.

The photograph No. 2, Mr. Simpson stated, showed that there were oscillations. Therefore, Dr. Chaffee showed that so far as any tests with outside plates were concerned you could take your choice. If you operated the apparatus abnormally to the extent of reducing the intensity of charge supplied to the condenser then you got diagram No. 1, but if you worked with a normal field
charge, then you would get No. 2. No. 2 was taken by 9566
Mr. Simpson with the mercury valve removed, as I remember it—no, I am wrong about that, with the antenna removed, and the mercury valve operating, and his interpretation of the figure was that since there was no antenna to which the energy could be transferred there were oscillations in the primary circuit and that diagram No. 2 proved it.

Q. Why did Mr. Simpson say that diagram, Fig. 2, in F. G. S.—4 proved it? A. Because the spot was deflected both sides of its zero position, and he reasoned that it went one side with one-half oscillation and the other side with the reverse half oscillation, and so on
back and forth, but Dr. Chaffee showed that with the per- 9567
fectly normal operation of the apparatus, connected in its normal way, you got diagram No. 2.

Q. With or without the antenna connected? A. With the antenna connected, everything perfectly normal, you got diagram No. 2, or you got diagram No. 1, just according to the voltage to which you charge the condenser, showing that the outside plates gave results that were entirely unreliable.

Then Dr. Chaffee disconnected the antenna and produced that No. 1 of F. G. S.—4, thus showing, according

to Mr. Simpson's interpretation, that when the antenna was disconnected there were no oscillations in the primary circuit. Dr. Chaffee performed various other tests in which he showed that what result was obtained by these outside plates depended entirely upon manipulation of the apparatus, and not upon the wireless apparatus at all, merely the field rheostat generator. The field rheostat is the means provided the operator for varying the extent to which he charges the condenser. Dr. Chaffee shows that simply by varying that, which is entirely remote from any wireless circuit, changing noth-
9569 ing whatever shown on the diagram of the Simpson mercury valve transmitter in the red or the black circuit, you could get either form of diagram that you pleased, and, therefore, the tests made with the outside plates could not in the nature of the case prove anything. I think that covers the tests, as I remember.

Q. Now, Mr. Waterman, referring to Chaffee diagram No. 1, and to defendant's exhibit F. G. S.—2 (Vol. 2, p. 1080), the Simpson mercury valve transmitter, I call your attention to the fact that the coil s in Chaffee diagram 1 is represented as a spiral, similar to the coil w in F. G. S.—2, is that correct? A. Yes, that is correct. It is a spiral in the actual apparatus.

9570 Q. Then in your diagram, plaintiff's exhibit No. 68, what coil of wire did you intend to represent corresponding to w in F. G. S.—2, and I think it is s in Chaffee diagram No. 1. A. The coil indicated by the two upper letters dd^1, or opposite the numeral 2 is the coil which Dr. Chaffee marks s and which in F. G. S.—2 is marked w.

Q. Is there any significance in the fact that those coils are represented as flat spirals, or as convolutions? A. No, none whatever. They might be either for actual utility, and whether one uses one form or the other is

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merely a matter of the space he has to put it in. I might just as well have shown a spiral in exhibit No. 68, and I will change it if it is desired, but the convention used merely illustrated an inductance, and it is equally an inductance whether wound as a spiral or helix.

Q. Now, in Chaffee diagram No. 1, where is the spark-gap shown? A. The spark-gap is shown at G.

Q. How is that represented in F. G. S.—2 and plaintiff's exhibit No. 68, that spark-gap? A. It is marked s in F. G. S.—2, and is illustrated as a plain open gap. It is indicated at G in exhibit 68, as a quenched gap, a series of gaps, just as Dr. Chaffee shows it. 9572

Q. And what corresponds in plaintiff's exhibit No. 68 to the inductance loop x in F. G. S.—2? A. The inductance loop x of F. G. S.—2 is shown at 2a in exhibit No. 68. It is an induction coil which is in the primary circuit only in the Simpson mercury valve transmitter.

Q. Where is that shown in Chaffee diagram No. 1, that inductance coil? A. It is shown at L^1.

Q. Now, where is the condenser c of F. G. S.—2 shown in plaintiff's exhibit No. 68, and in Chaffee diagram No. 1? A. The condenser c of F. G. S.—2 is shown at c, also marked dd^1, the lower pair of letters in No. 68. I will say that these letters dd^1 I had put there because of the designation of coupling. 9573

Q. Where is that condenser c shown in Chaffee diagram No. 1? A. It is shown a good deal in detail in Chaffee diagram No. 1. There are, as a matter of fact, seven of those condensers, seven condenser sections mounted in one box in the Simpson mercury valve transmitter. That is in order that the condenser may be varied for different wave-lengths, and the switch which is provided upon the actual panel varies the number of these condensers that are used, and the arrangement in which they are used, that is, their relations to one another. If we put two condensers in series, two equal condensers in series, their total capacity is only half what each was before, but if you put the two same condensers in parallel, side by side, their capacity is double what each was before. So that by arranging these condensers in various ways the defendant gets a wide range of variation of capacity, and this variation of capacity it uses in order to tune the primary circuit to the antenna circuit for each of the different wave-lengths for which the switch provides, that is to say, for four different wave-lengths.

9575 You pointed out to the court by indicating that with a pointer where that condenser is, I believe, but I would like to have it as a matter of record. A. It is shown at c in Chaffee diagram No. 1, the letter applying to the group which consists of seven connections. The various hieroglyphics at the left of that group of condensers are the switch points, and the arrangement which is shown is, I believe, the switching arrangement which exists for the 600 meter wave.

Q. Now, point out where the ground, which is indicated in F. G. S.—2, and earth in plaintiff's exhibit No. 68, is shown in Chaffee diagram No. 1. A. Well, it is indicated with the usual electrical designation of earth,
9576 a series of parallel lines of decreasing length, just below the letters h x.

Q. Where is the dummy shown in Chaffee diagram No. 1, to which you have referred? A. The dummy antenna is the portion beginning at the top of the inductance 1, and including the part marked "resistance 6 ohms", the part marked "variable inductance", the part marked "condenser .001 M. F.", the part marked "ammeter" and the connection x to earth, x x. I may say that this corresponds to the diagram given in the Bureau of Standards report, sheet No. 1, at the upper right hand corner.

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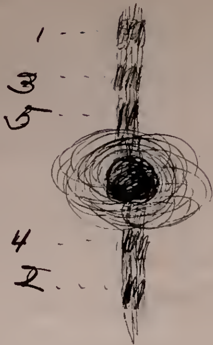
Q. Can you illustrate by a diagram what you personally saw on the screen of the Braun tube on the occasion of Dr. Chaffee's tests of defendant's Simpson mercury valve transmitter at the Cruft High Tension Laboratory of Harvard University? You have, I believe, in your previous testimony said that you saw two to two and one-half oscillations. Will you please diagrammatically illustrate that and call this drawing "F. N. W.—26" (reproduced opposite). A. I will endeavor to do so. (Witness draws diagram). Of course it is not easy to make a sketch showing the movement of
a spot of light particularly of that peculiar phosphore- 9584
scent type of light that one sees on a fluorescent screen, but most prominently one sees a central spot, which is surrounded by a haze, which is due to scattered rays, but doubtless also is more or less a matter of precision of ones own vision. Extending both sides from that is a streak of light, less intense a good deal than this central spot. The central spot is quite intense. In that pair of streamers which extend outward from the spot of light one can see, when the apparatus is operating stably, bright spots which appear like little bits of light in these two streaks, which denote the reversal, the point where the spot dwelt as it reversed.

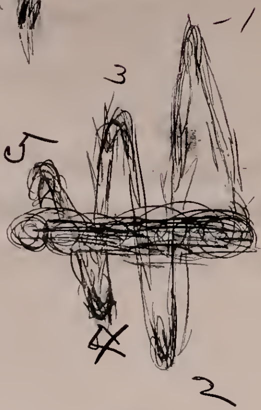
9585 Q. How many beads of light have you now shown on F. N. W.—26? A. I have shown three on one side and two on the other, and I have indicated them by numbers. The interpretation which I put on the showing is this: the spot is moved by current in the closed oscillatory circuit. Upon the breaking down of the spark-gap the energy which is stored in the condenser discharges through the circuit, then discharges back, and then forwards, back and forward, until the energy has all been consumed, either by transfer to the antenna or by heating of the parts. The spark-gap heats very rapidly in-

F.N.W 26.

1



2



3

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deed, in spite of the powerful blast of air that is all the time blowing upon it, showing that a good deal of energy is consumed in that gap. The first swing of the current, the first rush of current across the gap is the greatest, because all the energy is at that time in that circuit. It therefore causes the maximum deflection of the beam, which is deflected out a certain distance, and then as the current dies down comes back, but it stands for relatively a long time at that most remote point from the center.

Q. Indicated at point 1? A. Indicated at point 1. The current, by virtue of the peculiar properties of such
9587 a circuit, as has already been explained—I need not repeat—reverses and has its maximum swing in the opposite direction. Perhaps I had better change this marking, I will call that point 2 (Indicating). Then it swings, decreases again to zero and reverses again and goes this time to a lesser distance from the center, because the energy has been transferred to the antenna circuit by these swings, also some of it wasted in heating the gap. It goes to the point 3, then swings in the other direction and goes to point 4, and then swings back again and goes to point 5. What happens to it after that is lost in the general haze which really is rather greater than I have shown it, around this central spot. The fact that these
9588 deflections are produced by the action of the current flowing in the primary circuit, and that the deflections of the spot are in both directions from zero quite, shows, independently of the existence of any little beads, that the circuit is oscillatory, the current oscillates back and forth. The fact that the deflection in one direction is practically or nearly equal to the deflection in the other shows that there is more than one oscillation in that primary circuit of the Simpson mercury valve transmitter.

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Mr. Hughes: You mean more than one complete oscillation?

A. More than one complete oscillation. If there was only half an oscillation the spot would travel out to the point 1 and back again and stop. When it travels to point 2 and back again that makes a second half oscillation. When it travels to point 3 and back again there is a third half an oscillation, when it travels to point 4 and back again there is a fourth half an oscillation, and when it travels to point 5 and back again there is a fifth half an oscillation. Therefore, the diagram which I have drawn 9590
on F. N. W.—26, at 1, illustrates five half oscillations, two and one-half complete oscillations.

Q. Now, will you draw as Fig. 2 of F. N. W.—26 a diagram showing two and one-half oscillations on the time axis? A. I will do so.

Q. As you said you saw it during Dr. Chaffee's tests on the Simpson transmitter. A. I make a little circle in the center of the sheet, opposite the numeral 2, which is the spot of rest, point of rest. As the condenser is charged preparatory to a discharge, that is, as current flows from the generator through the transformer and the rectifying valve of the condenser to be charged, the voltage across that condenser builds up. Now, a small 9591
portion of that voltage was what Dr. Chaffee used to excite the internal deflecting plates of the Braun tube, in order to get the time axis. As that condenser charged up, therefore, the spot travelled over. I will indicate that fact by the general hazy appearance which a rapidly moving spot of light has. When the condenser discharges then the deflection from the zero position in the other direction to the position 1 which I have shown at diagram 1 of F. N. W.—26 occurs, and, of course, the spot immediately starts back to zero, because the condenser is now

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discharging. The free discharge of this pair of plates inside the tube is prevented, however, by the use of resistance in the circuit, which Dr. Chaffee has fully explained, so that it takes rather a long time for this spot to travel back. As the spot is traveling back in what is a horizontal direction on F. N. W.—26 it is deflected in what is the vertical direction on F. N. W.—26 by the coils, so that an effect somewhat like that which I have tried to indicate at 2 on F. N. W.—26 is produced. As the spot of light comes back it is deflected by the current. The first half of an oscillation I have indicated as up,
9593 the next half of the oscillation as down, the next half of the oscillation as up, the next half oscillation as down, and the next half oscillation as up, and I put the same numbers on the corresponding points in this diagram. That is some such appearance, as near as I can approximate to it, as that which one gets on the screen. In other words, one sees a train of oscillations according to the precision with which the apparatus is repeating itself, and it is astounding that any apparatus can work so accurately as to cause a spot of light to go over a path like that some ten or twelve thousand times, or certainly many hundreds of times, to get the visual impression.

The third test of this group which Dr. Chaffee made
9594 was the one on the antenna when the connection of the pair of deflecting coils was to an antenna lead rather than to the lead in the primary circuit. In this instance the time that it took the pair of deflecting plates within the Braun tube to discharge proved less than the total time of the phenomena, consequently, an effect was produced at one end, which I will show—(witness draws diagram). I have shown at 3, as nearly as I can, the effect produced on my eye in this test. Perhaps, to avoid confusion, I should have turned it the other way around, end for end, but I guess the court will follow it all right. The evi-

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dence is seen of the building up of oscillations in the antenna circuit, and I saw usually these three dots on one side and two on the other, with a general haze at this portion which was too great to see whether there were others or not, this central portion being relatively very, very bright. This was followed by a quite brilliant, relatively to some of these other lines, streaks of light which constitute what we are accustomed to call the envelope of the oscillation train. The evidence of the envelope on the other side was also quite distinct. I interpret this as follows; upon the discharge of the condenser,
current—which had been zero in the antenna—began to 9596
be built up by the transfer of energy from the oscillatory circuit. Upon the next half oscillation in the primary circuit this previously built up current was not only maintained in intensity but increased.

Q. In what circuit? A. In the antenna circuit. On the next it was not only maintained as great as before, but further increased.

Q. In what circuit? A. In the antenna circuit. In that next oscillation of the primary circuit the antenna current was not only maintained, but again increased, and finally was not only maintained, but again increased up to the ultimate maximum, and at this point the energy
was all transferred from the primary circuit and the 9597
oscillation of the antenna without any maintaining supply from the primary circuit was completed, and the motion of the spot of light along the time axis was necessarily slower and slower and slower, so that these oscillations are crowded together and the condenser actually began to charge up again from the supply source before the train of oscillations was quite finished, and that is the explanation or interpretation which I put upon this little re-entrant evidence of oscillation, being the heavier spot at the right of F. N. W.—26, diagram 3. I will

apply to Figure 3 of these diagram numbers corresponding to those which I have applied to the other two diagrams, it being understood that these numbers are in this case to represent the effect in the antenna of the similarly numbered oscillations in the other two figures.

Q. Now, Mr. Waterman, how does what you say you saw on the screen of the Braun tube during Dr. Chaffee's tests of July 3 and 4 at the Tuft High Tension Laboratory, and which you have illustrated in F. N. W.—26, compare with Mr. Kolster's drawing, K-5 (see Vol. 2, p. 1248), which I now show you, particularly Fig. 1 thereof.

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(Whereupon the court takes a recess until 2:00 o'clock P.M.)

July 19, 1916, afternoon session, 2 o'clock; continuation of proceedings pursuant to recess. All parties present as at former hearing.

F. N. Waterman, same witness, on the stand for further direct examination.

Q. (Mr. Betts) Now, Mr. Waterman, in answering
9600 the last question propounded just before recess, I will ask you to very briefly answer it, referring to Mr. Kolster's testimony in regard to his chart K-5, found at page 1145 of the record. A. Mr. Kolster's Fig. 1 of his diagram K-5 corresponds substantially exactly with the diagrams which I have drawn as Figs. 1 and 2 of my drawing F. N. W.—26. Mr. Kolster says——

The Court: What page are you reading from?

A. (Continuing) Page 1145 of the record.

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"Q. Referring now to your drawing K-5, if the circuit which you have described as a disturbing circuit or trigger circuit were characteristically an oscillating circuit, would you please show, in reference to your drawing K-5, what the indication would be with the Braun tube photograph? A. Well, if the number of oscillations which were occurring in that circuit were as represented in Fig. 1, the photograph would appear very much as this sketch indicates. There would be light spots at these various places, and at both sides of the zero point or the spot of light—both sides of the zero spot, symmetrically arranged.

"Q. If the disturbing circuit is not character- 9602
istically an oscillating circuit, how would it appear in the photograph? A. As in Fig. 2 of my chart K-5."

Fig. 1 of Mr. Kolster's chart K-5 shows a train of oscillations and, on the assumption that such a train of oscillations occurs in the primary circuit of the Simpson mercury valve transformer, he says, in the passage which I have just quoted, that the appearance of a straight line deflected spot upon the Braun tube screen would be that which is shown to the left in Fig. 1. There would be a central spot, which he marks A; with a de-
flection to the point B and the dot there indicating the 9603
dwelling of the spot as it comes to rest and reverses; then an excursion of the spot to the opposite side of the zero, to the point C, and a point of light at that point to represent the dwelling of the spot; then an excursion again to the other side and a spot of light at the point D; then an excursion to the other side and a spot of light at the point E.

As a matter of fact, that is exactly what was shown by Dr. Chaffee's tests and exactly what I have tried to represent as seen upon the screen, in my F. N. W.—26. In other words, if I had drawn, as would have been bet-

9604 ter, my Fig. 2 and my Fig. 1 in the same horizontal line, then the maxima or apex of the oscillatory curve waves projected across, as Dr. Kolster indicates, would have corresponded with these points.

Q. On your—— A. (Continuing) On my diagram F. N. W.—26, Fig. 1. I have put on Figs. 1 and 2 of F. N. W.—26, numbers 1 to 5, and it will be understood that the oscillations shown in Fig. 2 at 1 would, as Dr. Kolster indicates, project across if the diagram were made a straight line diagram, as it was in the first set of tests shown in Fig. 1 of F. N. W.—26. Spot 1 would
9605 then have been shown. 2 projected across shows spot 2 of Fig. 1. 3 projected across shows spot 3 of Fig. 1. 4 projected across shows spot 4 of Fig. 1. 5 projected across shows spot 5 of Fig. 1.

Therefore, what Dr. Kolster said would be shown if the circuit were oscillatory, and which he illustrated in K-5 was shown, as a matter of fact, by Dr. Chaffee in his Braun tube tests, and the tests, therefore, agree with this statement of Mr. Kolster regarding his Fig. 1 of his chart K-5.

Q. Will you please now refer to any sample of the negatives taken and produced by Dr. Chaffee showing the operation of defendant's Simpson mercury valve
9606 transmitter when all parts of the spark gap were in circuit and the apparatus was working normally at a single discharge per half cycle, state to the court what such negatives show? A. Negative No. 21 is a sample of the first set of tests of Dr. Chaffee, and it shows what is shown in my chart F. N. W.—26, there being the vertical line of light in which there appear a number of dots, three above and two below—with a possible third below which it is difficult to be sure of.

Negative No. 22 shows what I have shown in Fig. 2 of my chart F. N. W.—26, namely, a wave train, there being three above and two at least below, with a possible third.

These negatives represent the operation of the apparatus with all five gaps and with a single spark per half cycle.

Q. Mr. Simpson has testified, at page 1243 (Print p. 1370, Vol. 3) of the record, that sometimes the defendant Simpson mercury valve transmitter is operated to produce a plurality of discharges per half cycle, and Mr. Kolster's Bureau of Standards report, defendant's exhibit No. 10, shows that he so operated
the defendant's Simpson mercury valve transmitter 9608
at the Bureau of Standards at Washington, D. C. Will you, therefore, refer to any sample of negatives taken and produced by Dr. Chaffee when testing the Simpson mercury valve transmitter as it was operated to produce a plurality of discharges per half cycle, and exhibit such negatives to the court? A. Negative No. 24 is such a negative, and it shows three oscillations above the line and three below; that is six half oscillations, or three complete oscillations. It is like my Fig. 2 of F. N. W.—26, except there is another half oscillation.

The lower photograph of the three which are found on negative B. G., is the straight line deflection of the
spot as evidenced by a horizontal line through the cen- 9609
tral spot containing beads upon it on both sides of the center, and this bottom picture on negative B. G. was also taken by the first of Dr. Chaffee's methods, namely, straight line deflection only, when the apparatus was operated with two discharges per half cycle.

Q. Will you compare the results of the operation of defendant's Simpson mercury valve transmitter when this transmitter was operated with but a single discharge and a plurality of discharges per half cycle, as shown by Dr. Chaffee's negatives? A. An examination of the

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negatives which I have just produced and referred to, will show that the results are essentially the same. There is no material difference; taking negatives Nos. 22 and 24, for example, which show, in the case of 22, the operation with only one discharge per half cycle of the generator current, and comparing it with No. 24, which shows the results when there were two discharges per half cycle, shows clearly the fact that there is no difference in kind of operation at all,—the operations are the same sort.

Q. You may state whether Dr. Chaffee conducted any
9611 tests in your presence on July 3rd and 4th, and in the presence of the defendant's representatives whom you have named, on defendant's Simpson mercury valve transmitter when the primary or spark gap circuit of this transmitter was disconnected from the secondary or antenna circuit? A. He did. I have already outlined briefly what he did. He disconnected the antenna circuit, as I have described, and took wave train photographs of the behavior of the spot on the screen after exhibiting it for visual observation.

Q. Can you refer to any sample negative produced by Dr. Chaffee showing the operation of the Simpson mercury valve transmitter when the primary or spark
9612 gap circuit was disconnected from the secondary or antenna circuit, and exhibit the same to the court? A. I refer to negative No. 60, which was taken under the conditions mentioned, and which shows a number of oscillations, six or seven possibly, on each side of zero. I have not made any drawing showing this, but what was seen by the eye was what is shown in the negative, and differs from No. 2 of F. N. W.—26 only in that there were more oscillations visible.

Q. I believe you stated this morning that Dr. Chaffee conducted some tests on the Simpson mercury valve

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transmitter when the condenser c shown in defendant's exhibit F. G. S.—2 was left only in the primary or spark gap circuit, didn't you? A. Yes.

Q. Can you refer to any negative taken by Dr. Chaffee of the Simpson mercury valve transmitter when the condenser c, shown in defendant's exhibit F. G. S.—2 and plaintiff's exhibit 68, was only in the primary or spark gap circuit? A. This condition is represented in the top figure on plate D. T. This negative is somewhat underexposed, and to see the full extent of the straight line deflection on it, one has to look rather carefully, but it is apparent at a glance that the deflection is equal, sub- 9614
stantially, on both sides of the zero; that being the evidence of oscillations which is agreed upon in that case. The horizontal lines standing at both sides of the center shows the presence of oscillations, and in a proper light there can be seen two on each side.

Q. You mean by the zero or center, what portion, as represented in negatives D. T.? A. A very black, badly halated spot in the center is the position of rest of the spot and the position which it occupied at much the greater portion of the time. It will be understood that the spot is actually moving, as I have explained, only a small part of the time; consequently, the center of the spot has from five hundred to one thousand times the 9615
exposure of the rest, and the plate halates, which means that you get the same effect which you would get when you take an ordinary picture inside a room towards an open window—the outline is lost.

Q. Now, Mr. Waterman, I will ask you again to refer to the Bureau of Standards report, Mr. Kolster's defendant exhibit No. 10, and point out to the court what is there stated to be, and disclosed in the data annexed to the report, as to the oscillatory character of the primary of the Simpson mercury valve transmitter? A. The

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Bureau of Standards report (Deft's. Ex. No. 10) shows and states in a great number of places that the discharge in the primary or trigger circuit of the Simpson mercury valve transmitter was oscillatory in its character. In sheet 1, for example, are shown resonance curves taken from the apparatus, and the mere fact that such a curve as No. 3 of sheet 1, or No. 2 of sheet 1, can be taken, is of itself evidence of the oscillatory character of the discharge. Because, if the discharge were not oscillatory, then such a curve could not be obtained.

Further, upon this sheet appears, in the upper left
9617 hand corner, a statement of the decrement, and in the body of the report there are results of calculations tabulated, showing the decrement of the circuit as determined from this curve. Were the circuit not oscillatory there would, of course, be no basis whatever for any such calculation, and the calculations are based upon the theory of oscillatory currents developed by Bjerknes.

The same statement may be made of sheets 2, 3 and 5. In other words, the very fact that such curves are obtained and decrements calculated from them, shows that the circuit is oscillatory.

A further striking proof of the oscillatory character of the circuit is shown in sheet No. 3. This sheet shows
9618 how the apparatus behaved when the spark gap operation was interfered with; or rather, putting it the other way—the coupling was too tight for the quality of spark gap operation. The presence of beat waves, as they are called, is shown, and of course beats cannot occur except in oscillatory circuits.

At page 3, lines 25 and 26——

Q. Referring now to the report itself? A. Of the report itself—reference is made to "The oscillations existing in the trigger circuit."

Q. What does the report say in the passage you are referring to? A. On page 3, beginning at line 25:

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> "Both curves 2 and 3 are very broad"—the reference being to sheet 1—"indicating that the oscillations existing in the trigger circuit both with and without the antenna circuit connected, are non-persistent."

That is, there are comparatively few of them. That the existence of oscillations in the circuit is clearly recognized, again at page 4, line 9, the statement is made:

> "It is not to be understood, of course, that the oscillations existing in the trigger circuit decay in a logarithmic fashion."

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Again, at line 20, without reading the whole sentence—

> "Therefore, the oscillations in the trigger circuit die out much sooner than would be indicated by a measurement of the equivalent logarithmic decrement."

Again the existence of oscillations is recognized. I will continue with that quotation, because in the two lines below there is a similar statement—

> "In fact it has been observed by several investigators that the decay of oscillations in a circuit containing a spark gap and in particular a 9621
> short series of spark gaps, obeys a linear law rather than a logarithmic law. With linear decadence the oscillations actually cease at a definite time."

In other words, the oscillations in the circuit of the Simpson mercury valve transmitter are referred to just as oscillations in other circuits containing spark gaps.

I might recall the fact, which I need not repeat, I think, that in my former deposition I referred to authorities, particularly, if I remember to this Fleming book, 2nd edition, in which it is stated that circuits having a spark gap in them are more nearly linear in the decay of the oscillations than logarithmic.

Again, at line 29 and 30 of the same page 4, it is said:

> "Calculations based on the work of E. Taege applied to curve 3, sheet 1, indicate that the current in the trigger circuit drops to zero in approximately two complete oscillations."

That is, calculations based even on the assumption that there was no resistance anywhere except in the so-
9623 called trigger or primary circuit, gives two complete oscillations, and if there were no equivalent resistance anywhere else, why, obviously, the apparatus would be inoperative, because the transfer of energy from the primary to the secondary circuit is a large part of the effective resistance that the circuit has, and therefore, the oscillations must be greater in number than that so calculated, assuming the calculations to be correct.

Now, on line 30 of page 5, there is also another reference to the oscillations in the trigger circuit. It speaks of that—I will read the entire paragraph:

> "This is largely due to the fact that even without the use of a special type of a spark gap, the
> 9624 oscillations in the trigger circuit are comparatively highly damped, because of the small value of L over c."

Here the oscillations of the primary circuit are again referred to.

Again, paragraph beginning at line 10, page 6, shows the existence of oscillations. It says:

> "The data sheets attached herewith give the results of calculations made from the several trigger circuits resonance curves on sheets 1, 2, 3 and 5. These calculations were made to show

that these resonance curves do not have the same characteristic as those obtained from the logarithmically decayed train of oscillations."

Thus, referring to these tables we find that in every case, both the normal and the abnormal operation of the apparatus, oscillations are shown.

Q. Just point that out to the court, where it is shown in the data contained in the report just referred to? A. Taking the first table on page 7, which refers to curve sheet 1 of curve No. 2, taken when the antenna was not associated—just with the primary circuit acting alone, 9626
the logarithmic decrement as derived is equal to .521; from which ten oscillations would be inferred. Calculating on the basis of the Taege equation, the number of oscillations, on the assumption that the spark gap resistance is the entire resistance of the circuit, and that the conductors and the condenser have no resistance whatever, is three—that is to say, 2.9.

The second table, namely, at the bottom of sheet 7, gives the results with reference to the apparatus as operating normally, that is with the antenna associated and energy being transferred.

Here, of course, that is in the operative condition, the effective resistance of the primary circuit is largely 9627
enhanced by the coupling to the antenna circuit, and this coupling being constant gives the effect of a constant resistance.

From the tabular matter given, the logarithmic decrement as deduced is .712, from which, by the usual logarithmic formula, there would be seven and one-half oscillations. Using the Taege formula, assuming that all the resistance is in the spark gap, there is given in the last line of page 7, 2.1 oscillations—the actual number, of course, lies between these two figures.

Curve sheet 2, which is considered in the first two tables on page 8, was taken with the apparatus in an abnormal condition—not the condition of use. But, briefly stating the result, the logarithmic decrement is .52; indicating about ten oscillations. And the number of oscillations based on Taege, is again 2.7, or, substantially, 3.

From curve 3 it is given as .869. And the number of oscillations based on Taege is .17.

Here again, the circuit was coupled and, of course, the actual decrement must lie between the two, and the actual number of oscillations must lie somewhere between
9629 1.7 and 6.

The last two tables on page 8 have reference to curve sheet 5, and curve sheet 5 is taken with the mercury valve out of operation. It is entitled "Mercury tube short-circuited". There is shown with reference to curve 2—that is the operation when the antenna is removed—an average logarithmic decrement of .444, indicating about eleven oscillations from the logarithmic formula; and from the Taege formula 3.4 oscillations.

The last table relates to curve 3 on this sheet 5, when the mercury valve is not operating. There is mean decrement, logarithmic of .667, and a decrement based on Teage's equation of 2.2. Each circuit being coupled,
9630 and hence the useful work of radiating energy going on at that time, evidently a much larger portion of the resistance is constant and, hence, the actual number of oscillations lies somewhere between.

I call attention particularly to the table belonging to curve 3, sheet 5, found at the bottom of page 8, and curve 3, sheet 1, found at the bottom of page 7. The number of oscillations based on the Taege equation is 2.1 when the mercury valve is being used, and it is 2.2 when it is not being used.

That result, in other words, is entirely within the possible accuracy of the results, and it shows that there

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is no appreciable effect whatever of the mercury valve upon the behavior of the wireless telegraph circuit.

Q. Are you familiar with the Taege formula for calculating the number of oscillations upon the assumption of linear decrement, which was referred to by Mr. Kolster? A. Yes, I am familiar with the Taege article and the equations developed therein. I have never before seen them used by anybody.

Q. How does your calculation of the number of oscillations in the primary circuit of the defendant's Simpson mercury valve transmitter agree with Mr. Kolster's
using the Taege formula? A. Why, that, of course, is 9632
merely a matter of arithmetic; and taking Mr. Kolster's assumed average decrement, my results agree exactly. That is, Mr. Kolster's mathematical work is entirely correct in that deduction.

Q. You have, I believe, referred to the fact a moment ago, that there were authorities to show that the decrement of all circuits having spark gaps tended to be linear. Will you state what portion of the resistance of such a circuit as the primary circuit of the Simpson mercury valve transmitter is in a spark gap, and what part is in the condensers and the conductors, and how the number of oscillations should properly be determined?
A. That question is a very difficult one to answer. 9633

The resistances may be divided into three. That is, the total resistances may be divided into three parts, or four parts, if you choose. There are conductors in the circuit, and owing to the large ratio of c over L there is a large current in the circuit.

Now, the thing which causes the abstraction of energy is—or the thing which causes the decay of the oscillations, I mean, is the abstraction of energy. And this abstraction of energy is in proportion to the square of the current. Therefore, if the current is large its square

9634 is proportionately larger; and the current flowing copper conductors, therefore, causes an appreciable loss of energy, or conversion of energy into heat. That is one of the resistances—the resistance of the conductors.

Then the condensers always heat; and that is customarily referred to as a resistance. The condensers are said to have a certain resistance. Well, of course, they do. The material of which they are made—the metallic material of which they are made has a resistance, and furthermore there are losses in such condensers which also abstract energy and which are also spoken
9635 of as equivalent resistances.

Then there is the oscillation producer—the gap g. The resistance of that is a variable quantity. Initially it is infinite. It has no conductivity. If that were not so, of course, the apparatus would not operate. It is that non-conductivity which permits the initial charge. Put at a certain potential across the gap, that insulating material, the air which is between the terminals, gives way, and the current rushes across and the resistance is reduced to quite a low value. The resistance of the gap is, in a general way, inversely proportional to the current—the larger the current the smaller the resistance of the gap—there being a certain small constant factor,
9636 of course, in the resistance of the gap. As the current dies down in the gap, the resistance of the gap, therefore, increases; so that this resistance factor—this third factor of resistance in the primary or trigger circuit of the transmitter, is a variable one. The final element of resistance, which is not literally resistance at all, but which gives the principle damping or energy dissipation of the circuit, is the transforming effect due to the coupling between the primary circuit and the secondary circuit, which may be expressed as a resistance. This transforming effect is substantially constant. And so,

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there are three elements of resistance in a circuit, which are constant, and one which is variable.

Now, the Taege formula which has been referred to is one which would be applicable in a case where there were no resistances except that in the spark gap, and the spark gap followed this ideal, simple law of resistance variation.

If there were no variable resistance in the spark gap —if the spark gap should have its resistance kept constant—if there were no spark gaps there—then all the resistances of this primary circuit would be constant resistances and the decay of oscillations would be logar- 9638
ithmic—that is, theoretically, it would never end. But there would be a time after which the oscillations would fall into a small or negligible value, and we would then use the logarithmic formula for calculating that number, and that logarithmic formula is based usually on the assumption—not invariably, but usually on the assumption that the train has terminated when the last oscillation is one per cent of the value of the first. Sometimes ten per cent is taken; sometimes one per cent; sometimes some other per cent.

Now, as to precisely what the ratio between the constant and the variable resistances in such a circuit is, and specifically in the circuit of the Simpson mercury 9639
valve transmitter, I do not know. I only know that there is some constant resistance coming under the three classifications and some variable resistance.

Therefore the law of decay of the oscillations will be neither a logarithmic nor a linear law, but will be somewhere between. Hence it is certain that the number of oscillations in the circuit of the Simpson mercury valve transmitter is greater than that calculated on the Taege formula, as I have noted in the Bureau of Standards report; and also that it is less than the number of oscillations deduced from the figures of the report as to the logarithmic decay.

Q. But, in any event, as I understand you, there are two complete oscillations, quoting from the Bureau of Standards? A. Yes, the Bureau of Standards report shows that in the normal operation there is always two or more, even assuming that the resistance is all in the spark gap; in which case, of course, the apparatus would be inoperative as a wireless telegraph transmitter. There are always at least two, and the figures of the report indicate that there will be less than ten.

9641 Q. Then how do the tests conducted by Dr. Chaffee at the Cruft High Tension Laboratory, Harvard University, agree or compare with the data produced by Mr. Kolster, and embodied in the Bureau of Standards report of his tests? A. They are in perfect accord.

Q. What? A. They are in perfect accord. The actual number of oscillations which occur in a circuit cannot be indicated by the Braun tube method. All we see is that there are at least a certain number, and Dr. Chaffee's tests shows that there were always more than two; usually more than two and a half, and often more than three—that could be immediately presumed from the pictures on
9642 the screen. The Kolster Bureau of Standards report shows that there are always more than two decimal one (2.1) and always less than seven and a half—I said ten before—that was an error—always less than seven and a half. Somewhere between 2.1 and seven and a half. And this is exactly what one would necessarily conclude from the visual images upon the screen of the Braun tube in Dr. Chaffee's tests by all three methods of observation.

Q. The calculations of Mr. Kolster on the Bureau of Standards report, were on the basis of what logarithmic decrement, as shown in the report—I mean of the primary

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circuit—is it stated? A. In the body of the report it states seven-tenths, at page 4, line 6.

Q. Now if the apparent logarithmic decrement of the primary or spark gap circuit of a wireless transmitter was measured to be .5 when coupled to the antenna circuit, will you please calculate what the number of oscillations would be in that circuit, using the logarithmic formula, and also using the E. Taege formula, approved by Mr. Kolster? A. The number of oscillations by the logarithmic formula down to one per cent., would be ten and a quarter. The number of the Taege formula would be three
complete oscillations. And, of course, if the primary cir- 9644
cuit referred to were coupled to an antenna circuit, then the actual number must lie between these.

Perhaps an average or mean between them—just where between them would depend on the efficiency of the apparatus. If the efficiency was high then the number of oscillations would, probably, be nearer the logarithmic, and if it was low, nearer the linear, since the linear decrement implies waste.

Q. What does Mr. Kolster's report on the Simpson mercury valve transmitter show, as to the effect of the mercury valve and the number of oscillations of the primary circuit? A. The report shows, by comparing sheets
1 and 5, and comparing the tables at the bottom of pages 9645
7 and 8, respectively, that the oscillatory character of the circuit is not affected in any degree whatever by the mercury valve. The number of oscillations given while the mercury valve was working and the transmitter operating normally, is 2.1. That given for the operation normal, except with the valve removed, is 2.2. Being, as I have said, as close as the observations are accurate.

Q. How does this statement of Mr. Kolster, Bureau of Standards report, compare with the results obtained by Dr. Chaffee in the tests in your presence on July 3 and 4?

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A. It agrees exactly. That is to say, the tests made by Dr. Chaffee showed that it was entirely immaterial whether the mercury valve was operating or not. The primary circuit behaves in the same manner, substantially.

Q. Prior to the tests conducted by Dr. Chaffee on the Simpson mercury valve transmitter at Harvard University, did you personally examine the connections of the Simpson mercury valve transmitted and its associated parts? A. I did.

Q. I believe you pointed out this morning that the
9647 condenser, spark gap and inductance coils in the closed circuit, were similarly arranged in the Chaffee diagram No. 1, and in plaintiff's exhibit No. 68? A. Yes.

Q. Will you now state whether or not the mode of connections of the spark gap circuit in Chaffee diagram No. 1, is different from that shown in F. G. S. 2? A. No, it is the same—exactly the same.

Q. Is it then, in your opinion, an essential factor of Marconi's invention that the primary should oscillate so that, as energy is radiated by the aerial, the primary will persistently replenish the secondary with at least an equivalent amount of energy, and thereby maintain a radi-
9648 ating secondary? A. In my opinion, it is not. In fact, I think such an operation would be impossible. It is necessary merely that the energy should be transferred from the primary to the secondary circuit, the primary circuit maintaining and building up the energy in the secondary circuit until the energy is all transferred.

Q. And what is your opinion in regard to the transmitter illustrated and described in the Marconi patent in suit; as to whether or not the primary oscillations die down before the secondary oscillations are finished? A. The oscillations in the primary circuit must, of necessity, die down before the oscillations in the secondary are finished.

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Q. What is the fact, as shown by the tests at the Cruft High Tension Laboratory, of the defendant's Simpson mercury valve transmitter, as to whether or not the primary oscillations die down before the secondary oscillations are finished? A. That fact is very clearly shown by the tests, and it is illustrated, for example, in Figs. 2 and 3 of my diagram, F. N. W. 26. As I have explained, I inadvertently drew the figures 2 and 3 in opposite directions. It will be understood that the oscillation No. 1 corresponds to oscillation No. 1 in 3; the oscillation No. 2 of exhibit 2, corresponds to the oscillation No. 2, and 3 to 3;
4 to 4, and 5 to 5. Thus showing how the oscillation of 9650
energy in the primary circuit builds up oscillations in the secondary circuit. The energy being not only maintained —I mean by that this—as soon as the secondary circuit begins to oscillate it begins to radiate; it begins to lose—like pouring water into a leaking bucket, for example—and the secondary circuit not only pours energy in fast enough to make good the radiation, but also increases it, and then, as between 2 and 3, it not only maintains the energy, in spite of the radiating of energy into space, but increases it. And so with 4 and 5, until the energy has all been taken out of the primary circuit at sometime, depending upon just what the actual number of oscillations 9651
is, and the energy that is left in the system is then the energy that is left in the antenna. Some has been radiated, some is left, and if the apparatus is behaving in the normal, proper way, then that energy which is left is radiated out into space, as shown by the balance of the figure; it being understood that had this time period been uniform, these oscillations would be extended out here, there being, theoretically, no end of them—they would go on forever. As a matter of fact, the major part of the energy is gone in the first few oscillations.

Dr. Morecroft in his testimony calculated, if I remem-

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ber rightly, that sixty-five per cent. of the energy was gone in the first five oscillations; and ninety per cent. is gone after the first ten or twelve—I do not mean to say that Dr. Morecroft calculated that, but I could calculate it if you wanted me to—and therefore, what happens after the first few oscillations in the antenna circuit, is of trifling or no significance, because, theoretically, they are gone forever. It is the first few oscillations of the secondary circuit that are consequential, because those are the ones which contain the energy, and it is their energy which is received.

9653 Q. Did Dr. Chaffee conduct any tests in your presence on July 3rd and 4th, when the representatives of the defendant were also present, on the defendant's Simpson mercury valve transmitter, using the methods of tests adopted by Mr. Simpson at the Washington State University and as diagrammed in Mr. Simpson's diagram, F. G. S. 7? (Vol. 2, p. 1131.) A. Yes. As I stated in a former answer, the third set of tests that Dr. Chaffee conducted, he used outside plates, such as Mr. Simpson used, and connected them across the spark gap, as Mr. Simpson connected them. By "across" I mean, one plate was connected to, say the top terminal of the spark gap and the other to the bottom.

9654 Q. Now, I believe you have explained that fully this morning with reference to F. G. S. 4? A. Yes, I think I did.

Q. I will ask you if you will refer to any negatives which were taken by Dr. Chaffee using the methods which Mr. Simpson used at the Washington State University tests, and exhibit those negatives to the court, and explain them? A. I will refer to negatives B. O., which is a negative taken with antenna on, the operation perfectly normal.

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The Court: Both circuits connected?

A. (Continuing.) Both circuits connected, your honor; the apparatus working in its normal manner. The little dot at the top of the plate B. O. shows the position of the spot when the apparatus is not operating at all. The little spot at the bottom of the plate shows the position of the same spot when the condenser is maintained fully charged. In other words, Dr. Chaffee showed by that the fact that the spot does not deflect with the potential across the gap at all. When the condenser is fully charged the spark gap has across it the full potential, and yet these two spots at 9656
the top and bottom of the plate have identically the same position; showing that the assumption which Mr. Simpson made in his test, that the position of the spot would depend upon the voltage across the spark gap, is entirely incorrect.

The figure which is next to the bottom, it will be observed corresponds exactly to Mr. Simpson's No. 1 on his F. G. S. 4. It is a bright spot and a streamer of light extending out from it. Dr. Chaffee obtained that figure with one adjustment of the field rheostat; that is, with the condenser charged to a certain amount, and therefore, a certain voltage across the condenser.

The next one, being the second one from the top, it will 9657
be seen corresponds exactly to figure 2 of F. G. S. 4, and yet the apparatus was not changed at all.

Q. What is Fig. 2 of F. G. S. 4? A. It shows the central spot with the wave of light extending both ways from it, and a spray of light.

The Court: The wave seems to be in there longer on one side than on the other.

A. Yes, it is longer on one side than the other, and that is so also in Mr. Simpson's.

9658 F. N. Waterman—Recalled—Direct.

The apparatus was not operating any differently in one of these cases from the other, save that the condenser was charged to a higher voltage in the latter case. That is to say, when the apparatus was operating in a perfectly normal manner with one spark per half cycle—as good note as could possibly be obtained—everything perfectly normal, Dr. Chaffee obtained by this outer plate method, exactly the figure which Mr. Simpson has in F. G. S. 4 at No. 2, and which he said was taken with the antenna removed. And by having all four of these exposures on one plate Dr. Chaffee compares the several conditions, and
9659 shows that all sorts of results can be obtained with this outside plate method and with the connection across the spark gap, according to the particular adjustment of the generator voltage.

There are two reasons why this is so. Dr. Chaffee and the other gentlemen, Dr. Coffin, I believe, and Professor Cross and Professor Morecroft, testified that the use of outside plates is misleading and a dangerous expedient; that the result of outside plates cannot be correctly interpreted, and a further difficulty with Mr. Simpson's tests was that even if the outside plates had been perfectly reliable, he could not hope to have ascertained the facts
9660 by connecting across the spark gap, and for this reason: If F. G. S. 4, No. 1, were assumed to be taken by perfectly proper Braun tube technic, that is, if the structure and the operation of the Braun tube, as used by Mr. Simpson, had been in every way appropriate to the test, nevertheless Mr. Simpson's method of tests could not, by any possibility, have shown the facts as to the operation, and for this reason: If the tube operated as Mr. Simpson assumed it would; then, upon the charging of the condenser the spot would move from zero or the position of rest, out a certain distance. Now, the voltage across that condenser is something like from three to four thousand

volts. That is to say, the voltage across the gap, as long as there is not any current flowing at that time, is also from three to four thousand volts. This deflection, which is shown in F. G. S. 4 No. 1, and this enlargement of perhaps four inches, would represent some three or four thousand volts. The deflection of the spot would be proportional to the voltage, but the instant that the gap broke down there would be a large rush of current across there. The resistance of the gap would be a fraction of an ohm, and hence the total drop in volts across that spark gap would be just a few volts, maybe ten, maybe fifty, maybe one hundred—I do not know just what it would be; but 9662
in any case it would be so small that if this deflection represents, as it must, from three to four thousand volts, the deflection due to the oscillations would not show at all—would not be big enough to show.

It will be observed that this central spot is somewhat elongated. Now that elongation is sufficient on the adjustment that the apparatus must have had, to get the big deflection of the full condenser charge into the plate at all, sufficient to entirely cover and obscure the oscillations. Because the variations of voltage across the spark gap must, of necessity, be so small as compared to the maximum voltage across the gap before any current started to flow, that it could not be seen on the diagram at all. 9663

Therefore, even if the Braun tube technic had been entirely correct, even if it were perfectly proper to use such plates as he did, and even if Mr. Simpson had used perfectly proper plates and had operated the tube in a perfectly proper manner, he could not have found out anything about the apparatus by connecting it across the spark gap, as he did.

Q. Something has been said, Mr. Waterman, by defendant's witness Simpson, in regard to the connection on the spark gap circuit of the Simpson mercury valve trans-

9664 mitter to a point on the antenna circuit at or near what he calls a nodal point, and he has produced a diagram, defendant's exhibit F. G. S. 3. Will you please consider this exhibit F. G. S. 3, and state whether or not you agree with Mr. Simpson that there is a nodal point at that position, and give your reasons for any statement you may make? A. Well, I am entirely unable to follow Mr. Simpson's reasoning with respect to the alleged nodal point. His views on that subject are out of harmony with what I know about the matter and, as I understand it, with the authorities on the subject. However, I can only say that
9665 if there is such a nodal point Mr. Simpson's mode of locating that point is just the usual mode of adjusting wireless telegraph circuits. Mr. Simpson simply says that he adjusts the point of connection of the primary circuit to the place giving the best result, and that then he has it connected approximately at the nodal point. That is the mode of adjustment that was adopted in Cambridge by Dr. Chaffee, and hence his connection must have been at the nodal point, if there is any such thing. I have been unable, after carefully reading Mr. Simpson's testimony regarding this chart, to make anything intelligible out of it.

9666 In his Fig. 3 he apparently has the earth at a negative potential with reference to something. I infer that he has changed the potential of the earth in some way. Above the condenser he has a uniform positive potential, or potential of opposite signs, and he assumed that the huge condenser which is in this circuit is going to in some way bring about a nodal point. If I understand the authorities on the subject, they say a large condenser inserted in such an antenna acts, so far as the antenna is concerned, merely like a connecting wire, substantially. I think that Mr. Simpson is entirely mistaken as to there being any nodal point, but I think that the essential feature of the

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matter is that he connects, as he states, this point 2 and this point 2a on the two inductances in such a way as to produce the best results, and that best result is indicated by the biggest current radiating, and when he is doing that he is coupling two circuits together and tuning the two circuits to resonance one with the other, and those are the usual adjustments of a telegraph transmitter, and if we call a point a nodal point and say that we are fishing for the nodal point, the fact that the circuits are being adjusted to resonance and to proper coupling is merely stated in other words.

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RECESS.

Q. Will you please now examine defendant's exhibit F. G. S. 7 (see Vol. 2, p. 1131), and compare it with Chaffee diagram No. 1, and state how the apparatus was actually connected at the Cruft High Tension Laboratory by Dr. Chaffee during the tests. A. F. G. S. 7 shows in its figures the mode of connection of the Simpson mercury valve transmitter as used in the University of Washington tests. It also shows, save
for the connection of the Braun tube as shown at P, P^1 9669
and T, exactly the connections that were used by Dr. Chaffee at the Cruft High Tension Laboratory tests. The connections which Dr. Chaffee used for his Braun tube are shown on Chaffee diagram No. 1, and already been explained. Dr. Chaffee did also use the Braun tube connections which are shown in F. G. S. 7, that I have pointed out, and showed that with such connections no determination could be made as what the behavior of the local circuit was.

With reference to the remainder of the connections, that is to say, the connection with the Simpson mercury valve transmitter itself and the dummy antenna, these connections shown in F. G. S.-7 are precisely the connections used by Dr. Chaffee and shown in Chaffee diagram No. 1.

Further, Mr. Simpson's instructions as given in his testimony in this case were followed by Dr. Chaffee in making the adjustment, so that if any nodal point exists in this antenna other than that at the ground, then the apparatus in the Cruft Laboratory tests was so connected that the connection of the trigger circuit was to the nodal
9671 point of the antenna circuit exactly as Mr. Simpson has described, because the connections were made in exactly the same way and following Mr. Simpson's instructions.

Q. Instructions, you mean, in his testimony? A. In his testimony, yes. That is to say, in his testimony—I will quote if you desire—Mr. Simpson explained in detail exactly how this connection of the closed oscillatory circuit or trigger circuit is made to the spiral w, which is the spiral 2 of exhibit No. 68, or the spiral w of F. G. S.-2, exactly how those connections are made, and the connection to the little upset, as Mr. Simpson called it, which is in that circuit, in order that the connection may be to what
9672 Mr. Simpson calls the nodal point, and since Mr. Simpson's directions as given in his testimony were, as a matter of fact, followed closely and exactly the connection used by Dr. Chaffee was in this respect, as in all others, exactly that used by Mr. Simpson in the University of Washington test, and in the use of the apparatus in general.

Q. During the tests on July 3rd and 4th, when the defendant's representatives named by you were present, did they have an opportunity to examine the connections of the Simpson mercury valve transmitter and the Braun tube as set up by Dr. Chaffee? A. They did.

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Q. Did they do so? A. Yes.

Q. Was any suggestion made by Mr. Farnsworth or any of the representatives of the defendant there present, including Mr. Simpson, that the connection of the spark-gap circuit was not made to the nodal point? A. No such statement was made within my hearing, and I was right there all the time.

Q. I omitted to ask you, Mr. Waterman, if you could refer to any of the negatives produced by Dr. Chaffee which shows what you have represented in Fig. 3 of F. N. W.-26? (Page 3194.) A. Yes, there are a number of
such. 9674

Q. If you can, please exhibit the same to the court. A. There are a number of such negatives. I refer to BJ. This negative is a little faint, but the lines such as I have shown on my sketch 3 are discernible. I may say that the traces on the negatives are in no case as distinct as they were to the eye, because the eye is able to follow the variations of the apparatus better, and it will be remembered that unless the apparatus repeats with absolute fidelity the operations every time, that is to say, unless the spark occurs at precisely the same instant in the climbing or rising cycle of the generator wave, and unless it occurs at the same voltage each time, the time will not be super-
posed, and hence there is a tendency to blur. I think the 9675
court will see the tracings of oscillations building up in the antenna clearly in this negative, although they are not very dense. The negative is underexposed.

Q. Did you observe, Mr. Waterman, any difference in the mode of operation, or the way, rather, the Simpson mercury valve set operated at the Cruft High Tension Laboratory on July 3 and 4? A. Yes, the operation on July 3 was very much less regular than it was on July 4. In fact, the set towards the end of the day on July 3 became very nearly inoperative, and it was found on ex-

amining the gaps that two of them were short-circuited, and the gaps were all opened and examined, they not having been prior to that time disturbed since we received them. The gaps at the points where they were short-circuited had been very much roughened, there being high spots and low spots in them, which actually touched in the case of two of the gaps. There were two of the gaps that were in very good condition. The third one was intermediate, between the two that were in good condition and the two that were quite considerably burned. The gaps were then—that is, on the following
9677 morning, July 4th—were put in the lathe and trued up to a smooth finished surface, which surface was as perfectly true as lathe work can make it, and they were set at a separation of .006 of an inch, in accordance with the advice of Mr. Simpson, and I watched this operation, so that I can testify that the indications of the measuring instrument which was used and which was readable to about .0002 of an inch, was exactly within the essential margin of error .006 of an inch. After the gaps had been turned out the meeting surfaces of the outside plates were ground together with powdered emery in a way which had been described to me by Mr. Simpson, so that the joint was made air tight.

9678 Before these gaps were put in the lathe, I should have stated, and on the night of July 3, I think it was, the gaps were warmed with a Bunsen flame and finely powdered sealing wax was put into the grooves between the insulation and the brass, and melted in so as to be sure that the air gaps were tight, and thereafter, after the sealing wax had become cold and hard, I think it was the following morning, the operations in the lathe which I have just described took place, so that the gaps were certainly assembled with a separation, which to within a very small error indeed was .006 of an inch.

Q. And what is your opinion as to the condition of the apparatus on July 4, after these measures had been taken on the evening of July 3, as to whether or not it was in proper, normal operating condition? A. The apparatus was certainly in proper, normal operating condition on July 4th. The spark-gaps were in perfect condition. The whole apparatus was, as far as my experience with it enables me to say, in entirely perfect condition, and the note which it gave was an exceedingly good note, as good as was given by the same apparatus when operated by Mr. Simpson in the Kilbourne & Clark laboratory in Seattle in the presence of the court, at least as good. 9680

Q. Now, referring to the Marconi Patent No. 763,772, what statements do you find in the specification or claims of that patent to indicate that the energy—transfer from the spark-gap circuit to the antenna circuit is affected gradually during the successive oscillations of the spark-gap circuit? A. I find no specific statement on that subject at all, nor anything that implies just what degree of rapidity of transfer there shall be. The specification gives numerical examples whose operation is well known, and those numerical examples indicate or teach that the transfer of energy from the primary to the secondary circuit is in most cases very rapid, so that the energy having been once stored in the primary circuit, as I 9681 explained, oscillates a few times and transfers the energy to the secondary circuit.

The operation which I have shown in F.N.W.—26 is about the operation that would be expected from these examples given, in several instances.

Q. What description or instruction do you find in the specification or claims of the Marconi patent in suit, No. 763,772, which require that the oscillations shall be built up and maintained without the necessity of keeping the primary persistently oscillating? A. I find no statement

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in the specification at all that seems to meet the meaning of those words. The operative examples which are given I have already described, and the specification is in harmony with that description.

Q. In your judgment, does the Simpson mercury valve transmitter comprise but a single circuit? A. It does not. It comprises two circuits, namely, a condenser, inductance, spark-gap circuit, which is the primary circuit, and the open, radiating circuit which receives the energy from the primary circuit and radiates it to perform the useful work of a transmitter.

9683 Q. What is your opinion as to whether or not the primary or spark-gap circuit of the Simpson mercury valve transmitter is a reservoir circuit and co-operates with the antenna circuit on the principle of resonance? A. All of my experience with the transmitter, the report of the Bureau of Standards, and the tests of Dr. Chaffee, agree in showing that it is a reservoir circuit, receiving the energy and transferring it to the antenna. That is the meaning of the things seen on the screen as shown in F.N.W.—26, the meaning of the resonance curves which are given in the Kolster report, and the fact that the primary circuit operates in the same manner, differing only in degree whether the antenna circuit is connected
9684 or whether it is not connected, shows the same thing. Mr. Kolster's curves, given in the Bureau of Standards report, show that there is merely a little change of shape in this resonance curve when the antenna is attached, which corresponds to the transfer of energy to the antenna circuit. The operation of the circuit is the same in kind, that is, is oscillatory whether the antenna circuit is present or not, and by that oscillation it transfers the energy to the antenna and develops the high potential oscillations in the antenna which are essential to the transmission of waves.

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Q. Mr. Pickard, if my recollection serves me correctly, has testified that in his opinion the primary or spark-gap circuit of the Simpson mercury valve transmitter is an open circuit. Do you agree or disagree? A. Well, that is a matter of words. I agree—also I disagree. It is an open circuit in just the same way that every circuit, every primary circuit of every transmitter must be an open circuit. In Fig. 1 of the Marconi patent you see that the circuit is open at the point G. If it was not the apparatus could not operate. By virtue of the fact that it is open and that there is an infinite resist-
ance at the point G the transformer c charges the con- 9686
denser. Precisely the same condition exists in the primary or trigger circuit of the Simpson mercury valve transmitter. The spark-gap s is open. If it were not open there would be no operation. That is the preliminary stage. Now, upon the completion of the charging of the condenser the potential rises in the condenser to such a point as to break down the resistance of the gap. that is to say, the gap suddenly passes from the state of being a non-conductor, which causes the circuit to be open, into the state where it is a good conductor, causing the circuit to be closed. So, the words used depend upon which angle we are looking at it from. If we are look-
ing at it from the angle of charging we may say it is 9687
open; if we look at it from the angle of operation we say it is closed. But it is misleading to look at it and express it alone as an open circuit, because the term "open circuit" in the radio art has acquired the meaning of a circuit which radiates energy, and the closed circuit of the Marconi patent and the closed circuit of the Simpson transmitter do not radiate energy. I read from Mr. Simpson's testimony once today his statement that the closed circuit, trigger circuit, does not radiate energy, hence, in the proper use of the terms in the art the cir-

cuit should be called a closed circuit, or if called an open circuit it should be accompanied by explanation.

I pointed out in my former testimony from the Navy Manual what the terminology of the art is, and it is there shown that this spark-gap circuit is known in the radio telegraph art as a closed circuit, and the Simpson closed circuit is just exactly such a circuit as is shown in the Marconi patent in suit, Fig. 1.

Q. Mr. Pickard has also indicated in his testimony that the antenna circuit of the Simpson mercury valve transmitter is an electrically closed circuit. Do you
9689 agree or disagree with Mr. Pickard on that? A. Well, that again depends upon how you use the word. The statement is misleading as the words are used in the radio telegraph art. We may look at the antenna circuit of Fig. 1 of the Marconi patent as a closed circuit. It is a closed circuit in a certain electrical sense. For example, when the antenna has a charge developed in it the circuit must be closed in the sense that any circuit having a condenser in it is closed, because this elevator conductor has capacity with respect to earth, hence the electrostatic field exists in the condenser so constituted. In a similar strained sense we may say that the secondary circuit of the Marconi patent is closed, but it is very
9690 strained. The statement is quite misleading, because, as a matter of fact, all you have to do is to look at it to see it is not closed. It is open. It makes no difference how big a flat top we may have on it, it is still open, but the essential attribute of openness in wireless telegraphy is the power to radiate energy, and a circuit in a wireless telegraph system which radiates energy is called an open circuit. Hence, it is misleading to refer to it as a closed circuit, although by a certain use of terms it might be applied to any antenna circuit.

Q. You may state whether or not in your judgment

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that in the defendant's Simpson mercury valve transmitter the antenna circuit is charged directly from the mercury valve and not by way of a trigger, giving very briefly any reasons you may have for that opinion. A. That again is a matter of words, wholly a matter of words. If the antenna of the Simpson mercury valve transmitter, comprising the capacity area 2, the elevated conductor 3, and the various connections through to earth were charged from the mercury valve then the system would be so inferior in its operation that it would be of no use at all. Now, of course, portions of the circuit are common, just as in a chart which I have referred 9692
to that Mr. Pickard drew, where he illustrated different modes of coupling. We may couple by having certain elements in common, and in F. G. S. 2 we have a part of the coil w and the condenser in common, but that charging of the condenser which is done by the valve is the charging of it which is utilized in the primary or oscillatory circuit, and not the charging that is utilized in the radiation of energy, and for this reason; as the energy is stored in the condenser it is at a comparatively low potential, three or four thousand volts. I take it that the spark-gaps break down at about some such a voltage as that. At any rate that is very close to the fact. 9693
The total potential across the terminals, apart from any rise due to the presence of the condenser, is stated by Mr. Simpson to be 2200 volts. Well, we know that in a properly adjusted transmitter there is, due to the presence of the condenser in the power circuit, a rise of voltage and a rise of the order which would bring it to three or four thousand volts, is what would be expected. But the energy oscillating in the antenna circuit is at a very much higher voltage, and it is the coupling association between these two circuits that gives that rise of voltage. The books show approximate formulæ, for

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example, showing that the rise of voltage in the antenna as compared to the voltage in the spark-gap circuit is roughly in the inverse ratio of the square roots of their capacities, that being, of course, a very rough statement. The equations that develop that do not pretend to be accurate, but it is a function which depends in a measure upon the ratio of the square root of the capacity of the antenna to the square root of the capacity of the primary circuit.

Now, the capacity of the antenna is in the neighborhood of .001 of a micro-farad. That is, the capacity of
9695 the total antenna, including this condenser c, taking the whole structure just as it is found, with the condenser c in series, the total effective capacity of that antenna is about—that is, of the order of .001 of a micro-farad. But the capacity of the condenser c in this primary circuit is .070. It is seventy times the capacity of the entire antenna circuit. Hence, the potential, the charging potential of the antenna, is much higher in the antenna than it is in this local circuit, and this is by virtue of the transfer of energy from the local circuit to the antenna circuit.

It may seem to the court very strange that when we have small capacity in the elevated capacity area, and a
9696 large capacity in series with it, the capacity of the total antenna should still be small. That is because of the same curious property of the condenser which I was mentioning yesterday. When we put condensers in series we reduce their capacity, and if we put a large capacity in series with a small capacity we practically do not alter the small capacity at all, the large condenser acts merely as a conductor. I can give the formulæ and numerical examples if it is desired, but there will be no disagreement on the point that the total capacity of the antenna is of the order—that is, speaking of a ship's antenna—

is of the order of .001 of a micro-farad. The capacity of the condenser c is given in the Bureau of Standards report as .070, hence seventy times as great; hence the potential to which the antenna is charged is a potential much greater than the potential to which the primary circuit is charged, and if it were not so the set would not operate.

Another way of stating this fact is this: the charge imparted to an antenna is equal to one-half the product of its capacity times the square of the voltage. Now, the capacity is very small, of the antenna, as just stated.
We are to take one-half of that capacity and multiply it 9698
by the square of the voltage. If the voltage is also small evidently the total energy in the antenna will be trivial. It is the function of this organization set forth in the Marconi patent and found in the Simpson transmitter to charge a condenser of lower voltage, transfer that energy to an antenna. It is by virtue of that transfer and increase of voltage that we are able to get any considerable quantity of energy into space. It is, therefore, entirely incorrect and a misleading use of words to say that it is the antenna that is directly charged. It is the primary local circuit which is charged, and it by virtue of oscillations charges the antenna circuit.

Q. Mr. Pickard has, I believe, intimated in his deposi- 9699
tion that energy exists in the antenna circuit of the Simpson mercury valve transmitter in static form, and that such energy in the antenna is changed into kinetic form, that is, into oscillating current, by the discharge of the antenna caused by closing the trigger circuit. Do you agree with that statement? A. I do not. It contains the same completely misleading assumption which I have just been considering. If the antenna were, as a matter of fact, so charged, the system would be useless. It is the fact that the energy is put into a condenser in the local

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circuit, and that that circuit oscillates, and by oscillating raises the potential of the aerial circuit, develops or builds up, as shown in Fig. 3 of F. N. W-26, energy in the antenna that makes the apparatus useful.

Q. Mr. Pickard has, I believe, also indicated in his testimony that the sole function of the so-called trigger in defendant's Simpson mercury valve transmitter is to start the energy to oscillating, that is, to change from static form into oscillations. Do you agree with that statement? A. Well, I agree that it would be very nice if that could happen in any system, but it does not. That
9701 is, what I mean is this: the trigger—taking the Fig. 1 of the Marconi patent—the primary circuit for the purpose of charging must be open. If it were closed at the point G it would not charge and the device would be inoperative. Now, the gap remains open until the condenser is charged, then it breaks down. It would be very nice if its sole function could be to just break down and stay broken down while the energy was being transferred, and nothing objectionable happen from the other circuit, but that is not the way those devices are constituted, and they do not act that way.

It is equally a function of the gap, and it is a necessary function of the gap, that after the energy is oscil-
9702 lated in this circuit and has been transferred to the aerial circuit, that the gap should again resume its state of high resistance. Whatever statement one makes with respect to the Simpson mercury valve transmitter in that respect must, and for the same reasons, be made of the primary circuit and the spark-gap of the Marconi patent. The statement is one, in other words, which does not express the real facts, and misleads as to what the real facts are.

Q. Mr. Pickard has also indicated in his testimony in this case that in his opinion the antenna circuit of the Simpson mercury valve transmitter, the antenna circuit

is a reservoir of energy, or a reservoir circuit. Do you agree with that statement of Mr. Pickard? A. Well, that again is another case of stating a very small fraction of the truth as though it were the whole truth. Referring to Fig. 1 of the Marconi patent, for example, the energy is transferred from the closed primary circuit to the secondary circuit. As I pointed out by reference to F. N. W.-26, as the energy oscillates in the closed circuit—I may say F. N. W.-26 applies just as well to Fig. 1 of the Marconi patent as it does to the Simpson mercury valve transmitter—as the energy oscillates in the primary circuit energy is built up in the antenna circuit. When 9704
the energy has all been transferred, why, of course, the antenna circuit has got to hold that energy, in that sense, until it has subsequently been radiated by the decaying of the waves. There is, therefore, a remote sort of sense in which you can say the antenna is a reservoir circuit, but it is misleading, because it is not the customary way of looking at things, and because it expresses only a very small fraction of the truth, the truth being that the reservoir, the initial storage, is the condenser, and that energy is transferred to the antenna, and all of the energy which is not radiated by the antenna up to the time when the primary circuit stops, of course, is held until it does radiate, but to call it on that account a reservoir circuit is 9705
to state so small a fraction of the facts as to mislead. I have already explained why the fact that the condenser c is in the antenna does not make the antenna a reservoir circuit, so that I have covered both branches of the subject.

Q. You may state whether or not in your opinion the Simpson mercury valve transmitter of the defendant is of the direct charged type, or the so-called "single chunk" type. A. There is no such type, so far as I know, and the report of the Bureau of Standards and the investigation

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of Dr. Chaffee, the tests that I have made myself, all of the facts agree in showing that the Simpson mercury valve transmitter has a primary closed oscillatory circuit in which the energy is stored, in which it oscillates, from which it is transferred to the antenna circuit, and by the latter is radiated.

Q. Will you refer to Mr. Pickard's chart G. W. P.-18, which I now show you (see Vol. 2, page 663), and state what description there is in the Marconi patent in suit, No. 763,772, to warrant G. W. P.-18? A. G. W. P.-18 is what one might call a collec-
9707 tion of fragments. It is right enough as far as it goes, but if it is meant to imply anything more than it shows why then it is wrong. You might just as well describe part of any operation and let it go for the whole. It is incomplete. That is all. The same description applies to the Simpson mercury valve transmitter, and as regards the marking upon this chart, "Tesla type" I think it has been very clearly shown that he has not any foundation for that whatever.

The first diagram is marked, "Reservoir or condenser-circuit is charged (via coil d) with energy in static (stationary) form. Energy exists in condenser e in primary circuit, which is operably associated with antenna by coils
9708 dd¹". Well, that is all right as far as it goes. It applies to the Marconi arrangement, and it applies to the Simpson mercury valve arrangement for the same reason.

The second diagram says, "Transition stage. Beginning of transfer of energy to antenna from primary or reservoir circuit. Antenna about to begin to radiate effectively. Energy-transfer being effected gradually during the successive oscillations of the persistently oscillating reservoir circuit." All right, if you give the words the proper meaning it means exactly what is shown to be the operation of the Simpson mercury valve trans-

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mitter. If it means anything different it is wrong. The primary circuit of the Simpson mercury valve transmitter, like the primary circuit of the Marconi transmitter of Fig. 1 oscillates, and by that oscillation builds up energy in the antenna circuit. As soon as the energy begins to oscillate in the antnena circuit it begins to radiate, so it is proper to say that the transfer of energy having begun by the breaking down of the gap and the starting of oscillations, energy begins to radiate. It is perfectly proper, as it says in the second stage, to say, "Antenna radiating rapidly, due to vertical or open-circuit character, and therefore kept supplied by reservoir circuit. 9710
Stored energy of reservoir-circuit transmitted to the elevated conductor at the rate at which that conductor could effectively radiate it." All right enough if you know what it means. It means that the energy is being built up, transferred to the antenna circuit and built up therein, and as a partial expression of what is going on it is true enough, and it applies in the same way, with the same force and effect and meaning, to the Simpson mercury valve transmitter, the operation of which is identical, and for the same reason, because there are the same two circuits, the same proportion of elements in relation to the circuits, high ratio of C to L, the same sort of associa- 9711
tion of those two circuits. If the primary of one of them has a certain number of oscillations you may expect the same number in the other. There is nothing to differentiate the two.

Q. Now, I will ask you to refer to Mr. Pickard's chart G. W. P.-50 (see Vol. 3. p. 1682) entitled, "Pickard chart. Diagram of operation of defendant's 'Simpson' mercury valve transmitter," and state whether you agree with the showing, including the legends on that chart. A. (Continuing) Well, I would say that most of the statements are wrong, or so misleading as to be in effect wrong from my point of view.

The first stage shown is marked, "Charging antenna directly," and below it is stated, "Antenna charged directly from mercury valve, not by way of trigger. Energy exists in antenna circuit in static (i. e. stationary) form. No telegraphing possible until antenna energy is set in oscillation." As I have explained, if that was taken literally, it would simply represent an inoperative system, a system that would be of no substantial use. The energy must oscillate in this primary circuit and must thereby
9713 build high potential in the antenna circuit, or the system does not work. And the construction is such, as I think I have already fully explained, that the energy oscillates in the closed circuit and gradually transfers itself to the primary circuit, some of the energy being radiated meanwhile.

The Court: Do you contend that the antenna cannot be charged directly from the mercury valve?

A. The antenna is not charged directly in an operative sense. Of course, we agree that this condenser is a part of the antenna circuit, but, as I have pointed out, it is
9714 charged only to some 3,000 volts, whereas the effective charge of the antenna, depending upon just what antenna it is, is probably 60,000, or 70,000, or 80,000, or 100,000 volts, or maybe higher, in order to be effective. You can easily see by calculation, such as I suggested, that we could not get any useful energy into that antenna with a capacity of .001 and charged to 3,000 volts. Of course, if Your Honor understands, I am not denying any facts. That condenser is common to two circuits. That I take for granted. That is shown. We may assume that it is charged while in that antenna circuit to 3,000 volts, but

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that is not the working charge of that antenna. It would be quite foolish to say anything of that kind.

Q. What is the working charge of the Simpson antenna, as proved by the Bureau of Standards report, more than 3,000? A. The specific voltage to which that is charged is not stated in the Bureau of Standards report, but I can calculate it roughly, I presume, from the data which is given, although I would not do it on the spur of the moment. It would take some little time. But the voltage we know is of the order of many thousands of volts in these wireless telegraph antennae. That is why
the insulation has to be so extremely carefully made. 9716
Those wires that one sees stretching from mast to mast on a boat are very heavily insulated, and they must be heavily insulated because the voltage rises very high, and if the voltage did not rise very high we could not get any energy in it. That is, I mean amounts of energy that are of industrial telegraphic utility.

Now, this statement, therefore, which is attached to the chart marked "1st stage", states such a small fragment of the truth as to entirely mislead as to the real meaning of it. I disagree with it, therefore, from that point of view entirely.

The second stage is marked, "Changing form of en- 9717
ergy in antenna from static to oscillating current. Static energy in antenna is changed into kinetic (moving) form, i.e. into oscillating current by discharge of antenna-condenser caused by closing of trigger. The sole function of trigger is to start the energy to oscillating, i.e. to change from static form into oscillations. Then the trigger is opened." I have already discussed that and shown that that statement is not correct. It has a little fragment of truth. It is made to appear to be the whole statement and so it becomes a misstatement, because of the misrepresentation of the facts. What happens is

9718 shown by the Bureau of Standards report and the curves, and shown by Dr. Chaffee in the oscillograms. The energy is put in this condenser. When the spark-gap breaks down it is set to oscillating in the condenser circuit. Those oscillations are roughly indicated at 2 in F.N.W.-26. Those oscillations transform the energy into very much smaller currents of very much higher voltage in the antenna. The current in the oscillatory circuit is a very large current, maybe some hundreds of amperes at the maximum, at a maximum discharge voltage of only some three or four thousand volts. That becomes
9719 in the antenna some six, eight or ten amperes at a voltage of many thousands of volts, and it is the operation of the closed oscillatory circuit, containing the condenser, the inductance, inductance x, spark-gap s, and the conductor such as 7 of F.G.S.2 (Vol. 2, p. 1080) which transfers that energy to the antenna in that altered or transformed form. Then it is that the energy is in such form that it can be gotten into the antenna and can be radiated effectively. The statement, I think, under "2nd stage" contains so little truth as to be essentially wrong and misleading.

Under the heading, "3rd stage" it is said, "Trigger opened, leaving all the energy in antenna as reservoir
9720 commencing its persistent oscillations and persistent radiation. As soon as energy in antenna is set in oscillation there in the trigger instantly goes out of commission, the spark-gaps resuming their normal open condition; all the energy is completely stored in antenna circuits as the reservoir, at this point radiation commences. Oscillations never exist in trigger." Well, that was Mr. Pickard's opinion, which opinion was at variance with the report of the Bureau of Standards, which showed that oscillations do exist. The Bureau of Standards report, as I have fully pointed out, shows that the

number of oscillations existing in the primary closed circuit of the Simpson mercury valve transmitter cannot be less than 2.1, or more than seven and one-half, and they are, as a matter of course, as I have explained, somewhere between those two, because the one limit, two and one-half assumes that the energy is wasted or burned up in the spark-gap, which, of course, is not true. The assumption that they are seven and one-half assumes that the resistance of the spark-gap is perfectly constant, which we know is not true. Consequently, the true conditions are somewhere between. There is a number of oscillations, as shown by the report of the Bureau of 9722
Standards, which is somewhere between 2.1 and seven and one-half in the primary circuit, and Mr. Pickard was simply mistaken as to his facts, that is all. The inscription as to the 3rd stage is wrong. As I have already pointed out, the same state of affairs is proven by the tests at the Cruft Laboratory, and illustrated by what I saw on the screen, as I have sketched it in F.N.W.-26.

The 4th stage is inscribed, "Persistent oscillations in and persistent radiation from antenna of energy previously supplied to and stored in antenna or reservoir. Trigger yet out of service. All energy now in antenna. Latter now oscillating free from any disturbance which would result otherwise from a continuous reaction with 9723
a persistently oscillating primary circuit. Result is persistent radiation of waves of single i.e. pure frequency." Well, that is all right if you understand it properly. It is wrong if you do not, like the other inscriptions which I referred to on G.W.P.-18. If you interpret the term "continuous reaction with a persistently oscillating primary circuit" as implying something as regards the Marconi structure that does not exist in the Simpson mercury valve transmitter, it is wrong. The closed circuit of the Simpson mercury valve transmitter is a per-

9724 sistently oscillating circuit. It is a circuit which if we take away the antenna and put energy into it will persist in oscillating until all that energy is consumed within itself, because it cannot radiate to a notable or appreciable extent. Exactly the same reasoning applies, and for the same reason exactly, as applied to Fig. 1 of the Marconi patent, a closed circuit; the two are alike, big condenser, small inductance, spark-gaps in each of them. Now, energy put in that circuit will never get out of it if it is not associated with an antenna circuit, and that is what persistently oscillating means, and that is all it
9725 means. The Simpson mercury valve transmitter, like the Marconi transmitter of Fig. 1, has that circuit associated with a secondary or antenna circuit, so that the energy may be transferred out of it, and after the energy has been transferred, if the apparatus is operating properly then it is perfectly correct to say that the trigger circuit, the circuit d e G of the Marconi patent, Fig. 1, as I have shown it in exhibit 1, does go out of operation. The energy that is left in the antenna circuit is radiated. That statement is true of both.

Now, this lower inscription underneath stage 4 says, "Trigger yet out of service. All energy now in antenna. Latter now oscillating free from any disturbance which
9726 would result otherwise from a continuous reaction with a persistently oscillating primary circuit." That is true or not true, according to whether the spark gap operates well or does not operate well. The Bureau of Standards report in curve sheet No. 3 shows it is not true of the circuits of the Simpson mercury valve transmitter when we have a spark-gap that is not operating properly. The tests made of the Simpson mercury valve transmitter at the Cruft High Tension Laboratory showed that every time this apparatus got this spark-gap a little bit warm, then there began to be the beats, or

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interference between the two circuits, showing that in the apparatus as constructed the gap was not good enough to stand that amount of coupling, and what happens in that case, is shown in some of the photographs. For example, photograph 53 shows that by the two bulges on the curve, the interference between the circuits. That is precisely what happens with the apparatus of the patent in suit when the gap also is not working properly. So that to say that the Simpson mercury valve transmitter is something which does not suffer from that law is to state something that is not true, because when
the gap is cool enough so that one can keep his hand on 9728
the gap there is just exactly this interference that Mr. Pickard says does not exist. It is a defect, that is all. It is a defect that may come into any transmitter. But to try to draw a distinction between the Simpson mercury valve transmitter and some other transmitter, as, for instance, that described in the Marconi patent, which has the same elements, combined the same way, is simply to misrepresent the facts, in my opinion.

Chart G. W. P.—50 is built up on the assumption that there will be a single half oscillation only and always, and never anything else in the Simpson mercury valve transmitter. The evidence of the Bureau of Standards
report, the evidence of all the operation I have seen, and 9729
the evidence of the tests in Boston is that there never is any such operation under any condition, that there always is a train of oscillations in the primary circuit, and just such a train of oscillations as would be expected to result from the ratio of C over L that they have in that circuit.

Q. Referring just for a moment to F. N. W.—26, as I understand that Fig. 2 shows the decaying oscillations in the primary circuit, and Fig. 3 shows the building up and decay of the oscillations in the antenna circuit. A.

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That is right. The whole of Fig. 2 and the left hand end of Fig. 3, left hand half of Fig. 3, represents the cooperation of the two circuits while energy imparted originally to the condenser is oscillating in that circuit and gradually being transferred over into the other circuit. So we see by the decreasing height of these points one, two, three, four and five, the decreasing amount of energy in the primary circuit, and by the increasing height of points one, two, three, four and five, the increasing amount of energy in the antenna circuit. Then by the portion at the right we see (to a continually decreasing
9731 time axis, as I have explained), the oscillations that are taking place in the antenna circuit after the energy has been transferred. So, as I have explained, during this first period the energy which was resident in the local circuit, the condenser circuit, is oscillating, transferring itself over into the other circuit, making good what energy is being radiated in the meantime, and more than making it good, so that the energy is being built up, and then when the energy has been transferred, if the gap acts properly, performing its function in the circuit, then the reservoir circuit is through, its energy has been transferred and what is left oscillates in the antenna until it has been radiated. It is precisely the result of the ar-
9732 rangement set forth in the Marconi patent.

Q. What is the relation between the frequencies of the oscillations in Figs. 2 and 3 of F. N. W.—26?. A. The frequencies are identical, the circuits are in resonance with one another, as I have explained, as shown by the Bureau of Standards report, and as shown by the Cruft Laboratory tests. That is, the red circuit of Fig. 1 on exhibit No. 68 has a certain time period determined by the product of the capacity and the inductance. The antenna circuit in both cases has a certain time period, depending upon and determined by the product of the

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capacity and inductance. Those are the same, their products are the same, the circuits are in resonance. That means that the time periods of the circuits are the same, therefore, the oscillations taking place in the primary circuit are the same as the oscillations taking place in the secondary circuit. The two circuits have substantially the same time period.

Q. Mr. Waterman, I believe you have testified that you were an expert witness called by the plaintiff in the suits upon the Lodge and Marconi patents in suit, brought by the plaintiff here against the National Electric Signaling Company and decided by Judge Veeder, did you 9734
not? A. Yes, I did.

Q. Are you familiar with the patents and publications which were offered in evidence in that case? A. Yes, I was at that time.

Mr. Betts: If the court please, I would like to ask when I have closed my direct examination, or will close my direct examination, I would like to ask the witness if he will prepare between now and tomorrow a list of the prior patents and publications which were in the record in the suit before Judge Veeder in the National Electric Signaling Company case and be prepared to submit them 9735
to the court tomorrow, and I do so under the authority of the decision in "New York Filter Mfg. Co. *v.* Jackson", 112 Federal Reporter.

The Court: The list may be prepared and when it is presented we will consider what consideration should be given to it.

9736 F. N. Waterman—Recalled—Direct.

Thursday, July 20, 1916, 9:30 a. m. Continuation of proceedings pursuant to adjournment. All parties present as at former hearing.

F. N. Waterman, same witness, resumes the stand, for further direct examination.

Q. (Mr. Betts) Have you made, Mr. Waterman, a list of the prior patents and publications which were introduced in evidence in the suit of Marconi Wireless Telegraph Company against the National Electric Signalling
9737 Company on the Lodge and Marconi patents, decided by his honor, Judge Veeder? A. I have a hastily prepared list, which I would prefer to have more time to check, but it is as complete and as accurate as time permitted. I assume that I can just hand it to the stenographer instead of reading it.

The Court: Oh, yes.

Mr. Betts: And you wish to have it copied into the record as part of your answer, subject to correction?

A. Yes, if I may revise it subsequently.

9738 The Court: Yes; and let me suggest that it be likewise put in the files, so that if I have occasion to examine it I can refer to the files and get it without having to go into the transcribed notes.

Mr. Betts: Yes.

The Court: All right, you can mark it as Plaintiff's Exhibit 69.

(Document received in evidence marked Plaintiff's Exhibit 69 and copied into the record as follows):

PLAINTIFF'S EXHIBIT 69. 9739

Prior Patents and Publications offered in evidence in the suit of Marconi Wireless Telegraph Company vs. National Electric Signalling Company, in the United States District Court, for the Eastern District of New York.

Marconi Patent 586,193.
Marconi Patent 627,650.
Copy L'Electricista of Aug. 1, 1897.
Article in "Times," London, Sept. 23, 1896.
Clipping from Daily Chronicle of Dec. 14, 1896.
Article in Electrical Engineer of London, Dec. 18, 1896. 9740
London Engineer, June 18, 1897, Article by W. H. Preece.
Article by Prof. Slaby dated April, 1897.
Translation of Article by Prof. Slaby dated Berlin, 1897.
Rivista Marittima (Italian Technical Review, Aug., 1897).
Proceedings of Naval Institute 1899.
Marconi Royal Institue Lecture, June 13, 1902.
Pamphlet of Prof. Banti of 1897.
(Editorial) Electrician of Jan. 20, 1902 (page 346).
La Telegraphie Sans Fil by M. E. Ducretet.
Translation of Defendant's Exhibit No. 45, L'Electrician of October 8, 1898.
Partial translation of Ducretet Pamphlet, Defendant's Exhibit No. 70. 9741
Electrician of Nov. 27, 1891, Article by G. M. Minchin.
Electrician of London of June 26, 1891, Article by E. Branly.
Electrician of London of August 21, 1891, Article by E. Branly.
Pages 290 & 291 of the Proceedings of the Physical Society of London, Nov., 1889, Article by W. G. Gregory.
Physical Review of July, 1896.
Northup Articles, Kennedy Treatise on Wireless Telegraphy, 1906 Edition.

9742 Plaintiff's Exhibit 69.

Marconi Society of Arts Lecture, May 17, 1901.
Fessenden Electrical World, Article of July 29, 1901.
Fessenden Patents Nos. 706, 735, and 706, 736.
Tesla Patent No. 645,576.
Tesla Patent No. 649,621.
Thomson Patent No. 363,168.
Thomson Patent No. 525 of Jan. 7, 1898, British.
Pupin Patent No. 640,516.
Fessenden Patent No. 727,325.
Fessenden Patent No. 706,742.
Braun Patent No. 111,578 (German).
9743 Fessenden Patents Nos. 706, 737, and 706, 746.
Dolbears Patent No. 350,299.
Edison Patent No. 465,971.
Phelps Patent No. 312,506.
Marconi Patent No. 627,650.
Marconi Patent No. 12,326 of 1898, British.
Tesla Patent No. 454,622 (Reissue).
Ducretet Patent No. 9,791 of 1899 (British).
Ducretet Patent No. 288,067 (French).
Marconi Patent No. 586,193.
Braun Patent No. 1,862 of 1899 (British).
Braun Patent No. 22,020 of 1899 (British).
Thomson Patent No. 500,630.
9744 Hutin and LeBlanc Patent No. 23,892 of 1892 (British).
Pupin Patent No. 640,515 (Tuning).
Fessenden Article, Aug. 12, 1899, Electrical World & Engineer, N. Y., Vol. 34, No. 7, Aug. 12, 1899, pages 239-40.
Fessenden Lecture, Nov., 1899, A. I. E. E. Trans. for 11/22/99, Vol. 16, No. 12, Dec., 1899.
Tesla Book (Invention, etc., of N. T. N. Y., 1894).
Fessenden Article of 1897, Electrical World, N. F., Aug. 7, 1897, Vol. 30, No. 6, p. 151.

Plaintiff's Exhibit 69. 9745

Fessenden Article, July, 1899, Electrical World & Engineer, July 29, 1899, Vol. 34, No. 5, p. 167.

Fessenden Article, Sept. 16, 1899, Electrical World & Engineer, N. Y., Sept. 16, 1899, Vol. 34, No. 12, p. 421.

Fleming 1906 Book (London), Principles of Wave Telegraphy.

Fleming Book—Alternating Current Transformers.

Crookes Fortnightly Review Art of Feb., 1892.

Popoff Paper Read before Russian Chemical Society, 1896.

Ducretet's Lecture (L'Electricien), Oct. 8, 1898.

Lodge & Ebert Article (The Electrician), June 8, July 20, 1894. 9746

Hertz Book by Jones, 1893.

Preece Society of Arts Paper, Feb. 23, 1894.

Tesla's Franklin Institute and National Electric Light Association Lecture (1893).

Philosophical Magazine (July, 1889), Article by Lodge.

Lodge's 1892 Book.

Lodge's 1889 Book.

D'Arsonval's Paper.

Ducretet Pamphlet (Oct., 1899).

Pupin Article May, 1893 (Tuning).

Fleming Paper of 1904.

Thomson's 1892 Article. 9747

Branly's 1891 Article.

Reissue Patent No. 12,115.

Fessenden Patents Nos. 706,738, 706,739, 706,740, 706,741, 706,742, 706,744, 706,745, 706,746, 706,747, all of August 12, 1902.

Fessenden Patents Nos. 715,043, 715,023, of Dec. 2, 1902.

Fessenden Patents Nos. 727,326, 727,329, 727,330, 727,331 of May, 1903.

Fessenden Patents Nos. 730,753, 731,029 of June, 1903.

Fessenden Patent No. 742,780 of October 27, 1903.

9748 Plaintiff's Exhibit 69.

Fessenden Patent No. 753,863 of March 8, 1904.
Marconi Patent No. 12,039 of 1896 (British).
Stone Patent No. 714,756.
Marconi Patents Nos. 624,516, 650,109, 650,110.
Slaby-Arco Patent 1898 (German).
Blondel Patent of 1900 (British).
Fessenden Historical Article, Proc. A.I.E.E. July, 1908.
Bonnett & Bradshaw Thesis 1897.
Navy Manual (1911).
Swinton's Letter, The Electrician, Oct. 22, 1897.
Thomson's Journal of Society of Arts Paper (1898).
9749 German Edition of Tesla Book.
Popoff Article No. 2, July 1896.
Scientific American Sup. 1898, Vol. 46, p. 18, 874, recognizing Ducretet.
Tunzelmann Article (Smithsonian Inst. Report 1890).
Popoff letter 1897 Electrician, Dec. 10, 1897.
Electrician, letters relating to Marconi, June 11, 1897.
Tesla's I.E. Lecture (1892).
Pupin Article, American Journal of Science April, 1893.
Pupin Article, American Journal of Science, Nov. 1894 Vol. 48.
Pupin Article (Trans.) A.I.E. lecture of May 18, 1894.
9750 Pupin Article of Dec. 1894 (Am. Journal Science Dec.) 1894, p. 473, Vol. 48.
Electrical Review Article of 1897 (Elect Rev. June 4, 1897, Vol. 40, pp. 765 to 780.
Extract from Elec. Engineers Journal of 1898 (Jour. of Inst. of Elect. Engineers, Vol. 27, London, 1898, p. 948).
Houston & Kennedy Article of 1897 (Elec. World of N. Y., July 10, 1897, pp. 35 and 36).
Electrician Vol. 39, p. 736, Oct. 1, 1897.
Tesla's Electrical Engineer Editorial May 31, 1893, of the Electrical Engineer.

Plaintiff's Exhibit 69. 9751

Preece 1897 Royal Institution Papers.
Tunzelmann Book.
Marconi I.E. Lecture of April, 1899.
Marconi 1900 Royal Institute Lecture.
Marconi Society of Arts Lecture of 1901.
Marconi 1902 Royal Institute Lecture.
Marconi 1905 Royal Institute Lecture.
Elihu Thomson Article Electrical Engineer 1899.
Henry's Review March, 1897 Article by Kempster B. Miller.
Electrical Review of June, 1897.
Tesla Article Electrician, 1893, Jan. 6, 1893. 9752
Lagergren Article Analen der Physik & Chemie.
Bjerknes Article of 1891 Anlender Physik.
Marconi Lecture, 1902.
Navy Manual, 1903.
Lodge's Later Book."

Mr. Betts: I desire to offer now in evidence if the court please, plaintiff's exhibit heretofore marked 68. I offer that and ask it to be so marked.

The Court: It will be admitted.

(Received in evidence and marked "Plaintiff's Exhibit 68")

Mr. Betts: I also desire to offer in evidence 9753
the drawings made by Mr. Waterman, heretofore entitled "F.N.W.26".

The Court: It will be admitted.

(Received in evidence and marked "Plaintiff's Exhibit 70.)

Mr. Betts: The direct examination is closed.

CROSS EXAMINATION.

Q. (Mr. Farnsworth) Will you please, Mr. Waterman, compare the list which you have just presented of the defenses in the suit on this patent against the National Electric Signalling Company before Judge Veeder, with the list of defenses, patents and publications in this suit at bar, and point out then which of the latter are new defenses which were not included in said suit against the National Electric Signalling Company before Judge Veeder.

9755 Mr. Betts: I think that is a thing for counsel for defendant to do and not to ask the witness to compare them.

Mr. Farnsworth: If your honor please, that is what the witness was asked to do in the National Electric Signalling Company case and it is therefore competent for him to do it in this case. Of course the showing would be of a very materially large number of most important new defenses in this case, and if the counsel is permitted to show by this witness what the defenses were in that case, clearly we should have the same privilege.

9756 Mr. Betts: That is a matter of comparison, if the Court please, which counsel should make.

The Court: I believe it would be of much assistance to me in examining the matter, perhaps, if I had the witness' idea as to the defenses, if this will not be too prolonged.

Mr. Farnsworth: I am entirely willing for him to take his time on them, and not produce it just now, but later.

The Court: I do not want him to take too much time, but I believe it would help to elucidate the matter, perhaps, in the court's mind, if I had the witness's conclusion in relation to it, rather than counsels'.

Mr. Betts: I have no objection to his preparing a list of the prior patents and publications which have been referred to by Mr. Pickard, but your honor will recall that there are a large number of patents and publications introduced in evidence by Mr. Farnsworth, to which no reference was made, and to which I object.

The Court: I understand about 150, if I remember right now.

Mr. Farnsworth: Of course that is all right, 9758
what has been referred to as material defenses, of course——

The Court: If Mr. Waterman at some time, if he is not prepared now, make such a comparison, he can give such observations as might benefit the court.

The Witness: I can be prepared tomorrow morning.

The Court: All right, tomorrow morning.

Q. (Mr. Farnsworth) In that suit on the Marconi and Lodge patent before Judge Veeder, there was no demonstrations or tests made by the defendant on either the 9759
Lodge patent transmitter or the Telsa transmitter, was there? A. I think not.

Q. In that case, an appeal was filed, was there not, as you recall it? A. If I ever knew, I have forgotten.

Q. And that appeal was dismissed as the result of an exchange of licenses between the plaintiff and the defendant in that case?

Mr. Betts: I object to the counsel testifying; if the witness has knowledge he can state it.

9760 F. N. Waterman—Recalled—Cross.

The Court: The witness may answer; that is, if he has knowledge on the subject.

The Witness: Really, I do not know, Mr. Farnsworth. I may have known, but I have quite forgotten.

Mr. Betts: I am perfectly willing to state, on the part of the plaintiff, that an appeal was taken by both sides in that case, and that the appeals were dismissed and suits have been settled by an exchange of licenses between the Marconi Company and the National Electric Signalling Company
9761 pany under certain patents which they owned.

The Court: Very well.

Mr. Betts: I make that statement in order to shorten the cross examination.

Mr. Farnsworth: And I will ask either the witness or counsel, if it is not a fact that the Fessenden high spark frequency patent, under which the plaintiff Marconi Company obtained the license from the National Electric Signalling Company in that exchange of licenses, was not thereafter invalidated or not held to be infringed by his honor Judge Mayer?

9762 Mr. Betts: If the court please, I do not think it is material. I am perfectly willing to state the facts in that regard.

The Court: Yes.

Mr. Betts: The National Electric Signalling Company had, previous to this time, brought suit in the third circuit against the Telefunken Wireless Telegraph Company of America upon Tesla patents Nos. 918306 and 918307, commonly called the high frequency patent. The validity of those patents had been sustained by the Circuit Court of Appeals for the Third Circuit in an opinion ren-

dered by his honor Judge Buffington. Subsequently to that decision the National Electric Signalling Company brought a suit in the District of New Jersey upon the same Fessenden patents against the Marconi Company, and obtained a preliminary injunction against us. After the preliminary injunction had been obtained the parties went ahead with their defense. Therefore, when Judge Veeder had sustained the Lodge and Marconi patents and put the National Electric Signalling Company under injunction, and as we, the Marconi Company, were
under injunction on the high frequency patent, the 9764
two litigations were both settled and an exchange of licenses was arranged between those two parties, in order that both parties might supply to the public the most efficient form of apparatus. Subsequently, as I am informed—I was not counsel in the case, so I cannot state from my own knowledge, but counsel has asked me—the National Electric Signalling Company brought a suit against the Atlantic Communication Company in the Southern District of New York upon those same Fessenden or so-called high frequency patents Nos. 918,306 and 918,307, and that case was litigated by both
parties and, in January, I think, last, his honor 9765
Judge Mayer dismissed the appeal. I have forgotten whether or not it was non-infringement or invalidity.

Mr. Farnsworth: Both.

Mr. Betts: Well, he dismissed the appeal anyway, deciding for the defendant, and subsequently to that date, the defendant has taken an appeal to the circuit court of appeals in the Second District. Those are the facts as I know them.

The Court: Proceed.

9766 F. N. Waterman—Recalled—Cross.

Q. Referring to G. W. P. 18 (Vol. 2, p. 663), as you did yesterday, and the figure at the right entitled "Second stage," this being an exposition by Mr. Pickard of the construction and operation of the Tesla and Marconi type, or consistently operating type of radio transmitter; will you please explain carefully in detail what is meant by the operation as described, as follows: "That the stored energy of the receiver circuit transmitted to the elevated conductor at the rate at which that conductor could effectively radiate"?

Mr. Betts: Before you answer, I would like to
9767 say that, if my statement in regard to that litigation is not correct, and Mr. Farnsworth was in it, I will be very glad to have him make any suggestion.

Mr. Skeel: I think that was substantially correct. I may say that the only reason we consider that material was this: So that undue influence or importance might not be given to prior litigation. It may or may not have been adequately defended. It is obvious that the expense of litigation of this kind is so enormous that it is not every litigant who can defend a case, but Mr. Betts' statement
9768 was correct.

Q. (Mr. Farnsworth) What I would like is to have you explain what that means, that expression "Energy transmitted to the antenna or elevated conductor at the rate at which that conductor could effectively radiate." A. That language is, or at least purports to be, on the diagram G. W. P. 18, Judge Veeder's language.

I have not got Judge Veeder's opinion before me and I do not remember the text of the statement in his opinion sufficiently well to interpret it. The facts, I think, I have stated. Of course I would be glad to state them as fully as you desire.

Q. What does that mean, "At the rate at which that conductor could effectively radiate"; what is meant by that?

Q. (Mr. Farnsworth) You remember, Mr. Waterman, you used that same language in testifying, repeatedly in the case, and which Judge Veeder followed in his opinion? A. I do not remember it.

Q. You do not remember it? A. No.

Q. Then do you care to answer it. As a matter of fact, Mr. Waterman, in that Tesla-Marconi Company pat- 9770
ent of the primary oscillating system, is it or is it not a fact that the primary supplies to the antenna, and transfers the energy to the antenna at a rate at which the antenna could operate? A. I do not know of any Tesla-Marconi primary.

Q. You may answer my question? A. Well, if you ask a question that means something, I will.

Q. Do you decline to accept the question in its present form? A. I decline to answer your definition; if you will ask me something which you specify definitely I will be very glad to answer it.

Q. No, sir. You may evade every question that I
ask you if you please. Now, Mr. Waterman, would you 9771
give me what you understand to be the meaning in the trade of the expression "impulse transmitter" or "impulse excitation transmitter"? A. I do not understand that it has a trade meaning. The term is used in the art with more or less vagueness, which vagueness the Institute of Radial Engineers undertook a year ago to clear up, and their definition, as I remember it, is that it is a transmitter in which the coupling of the two circuits is so properly proportioned to the qualities of the gap that the energy, having once been transferred to the

antenna circuit, does not return to the primary circuit.

Q. From what do you understand such a transmitter is distinguished? A. It is distinguished from a transmitter which is too tightly coupled for the qualities of the gap. There was, at one time, a good deal of careless operation of wireless transmitter, in which the couplings were made too tight, and as there was no particular regulation, and there was not on the part of operators a great deal of intelligence the result was that the operation was conducted in a very defective and inefficient manner, and the law, in fact, stepped in to insist that the
9773 transmitter should be properly coupled so as to effect the transfer of energy properly. And that term, "impulse transmitter" has been applied to the properly associated coils. It is usually applied, however, that is it usually designates a transmitter which has a quenched gap of some kind or other, that is a gap which more easily prevents this transfer of energy back from the antenna to the primary circuit, such as a rotary or a stationary multiple spark gap. The advantages, as I explained, being that these gaps cool and so enable the gap to regain its non-conductive state.

Q. And what practical advantages do you understand are possessed, if any, by a transmitter of the impulse
9774 type, as distinguished from transmitters of the Marconi type of the patent in suit?

> Mr. Betts: I objejct to the form of that question in that it assumes that the Marconi patent is not an impulse transmitter—the witness has not so testified.
>
> The Court: I will let him answer it and allow your objection to stand.

A. There is no such distinction. To illustrate what I mean, I refer to the Simpson transmitter; that Simp-

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son transmitter is an ordinary wireless transmitter. It has no peculiarities which in any way operatively distinguish it. That transmitter operates to give one or the other of two types or sorts of picture when we get its operation on the screen. That is what I called the court's attention to yesterday. If I can find the pictures I will illustrate exactly what I mean. Now, if your honor will compare those two negatives——

Mr. Betts: What plates are those, Mr. Waterman?

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A. They are Nos. 22 and 53.

The Court: This says, "53, No. 1".

The Witness: That "No. 1" merely refers to the wave length at which the switch is set.

These two pictures represent the same apparatus in the same adjustment. Nothing whatever done to it; operating in the same way, but in one instance, namely that of 22, the spark gap was operating in its cool condition at the start of operation. In the other case the gap had warmed up a bit, not heated, simply become warm, as it necessarily would become in ordinary, moderate use. 9777

Now, as I understand the question, Mr. Farnsworth designates the action shown in 22 as one type, the impulse type, and that is shown in 53 as the Marconi type. As a matter of fact, it shows no difference of type whatever. It is the same apparatus, without any change of any kind in it; the apparatus being merely coupled a wee bit tighter than the spark gap is able to take care of.

The Court: Which is the tighter?

9778 F. N. Waterman—Recalled—Cross.

The Witness: It is the same coupling in both cases.

The Court: Oh.

The Witness: It is the same coupling in both cases. If the coupling were a little bit looser, then both pictures would be alike but it is only when the gap is perfectly stone cold that it is able to stand the coupling that it has got, so that the normal operation of the Simpson transmitter is 53, namely, what Mr. Farnsworth calls the Marconi type, but it is, in my opinion, perfectly absurd to
9779 say that there is any difference in the type whatever. It is the same apparatus, and it is operating in the same way.

Q. Then your opinion is that there is no difference in principle of construction or operation whatsoever between the transmitter of the impulse type and a transmitter of the Braun or Tesla-Marconi type, wherein, in the latter, the oscillations are persistent in the primary circuit—it is your opinion that there is no difference in principle of construction or operation whatsoever—is that your opinion?

9780 Mr. Betts: I object to the form of the question, in that the counsel assumes to testify instead of the witness.

The Court: I will take into consideration the form of the question—you may proceed.

Mr. Betts: I do not want to interrupt, only I want to make it a matter of record.

The Court: I understand.

The Witness: I cannot go into a discussion of the Tesla apparatus and the Braun apparatus. It is utterly impossible in one question for me to consider those patents. Therefore, I ignore them,

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and therefore I will answer the question as though they were not in it. That there is no difference in the coupled tuned circuit transmitters, as usually referred to in the art, whether you call it a coupled tuned circuit transmitter or whether you operate it with the impulse transmitter; it is your opinion there is no difference whatever in principle of operation or construction between the impulse transmitter and the coupled tuned circuit transmitter?

A. Absolutely none. And in that respect I am agree- 9782
ing with the defendant's witness, Mr. Stone. Mr. Stone has classified the transmitters, as I remember, entirely correctly. He has the quenched gap transmitter of the Marconi Company and the National Electric Signal Company and the Telefunken Company and the various Telefunken Companies, which are quenched gap transmitters operating on what the trade catalogues call impulse type of coupled tuned circuit transmitters, and that is just what they are. And Mr. Stone distinguished this defendant's Simpson mercury valve transmitter on the basis that it was, as I remember it, what he called a single chunk type. There was only one half impulse in the
primary. That was Mr. Stone's only mistake. Because 9783
the Bureau of Standards report and the Dr. Chaffee tests and all tests are made——

Mr. Skeel: (Interrupting) Just a moment. I object, so that the record will not be misstated, or misunderstood—Mr. Stone did not testify a single word or sentence in regard to the Simpson transmitter. I say that, and the record will bear me out.

The Witness: I do not mean that Mr. Stone testified specifically as to the Simpson transmitter,

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but he did make the classification which I have stated, and that classification is correct.

The change of the form of the spark gap itself does not in any way alter the mode of operation of the apparatus. We may get, with any spark gap, any possible mode of operation with any other spark gap, it is merely a question of proper adjustment.

Mr. Farnsworth's question implies that any transmitter which is in bad adjustment, which is operating improperly, is a transmitter of the Marconi type, and
9785 any transmitter, without any change whatever, that is operating properly is the impulse type.

Mr. Stone's classification was an entirely rational one, if you assume his understanding of the facts. All of these transmitters, regardless of their quenched and other so-called type of gap, are said by him to be coupled tuned transmitters. The Telefunken, which has the stereotyped, so-called, quench gap in all its various forms, is classed as coupled tuned, and it is a correct classification—and it is the only correct classification which has appeared in this case on the part of the defendant.

Mr. Farnsworth would say, in other words; we make
9786 no change in the apparatus whatever; that apparatus at one moment was the apparatus of the Marconi patent and in another, it was an entirely different type. Mr. Stone's classification makes it the same type all the time, which is correct, obviously.

Q. Now, comparing the impulse transmitter with the transmitter of the Marconi patent in suit, is it your opinion, in short, that there was no difference in the principle of construction or operation, between them? A. I have stated what the difference in the ordinary operation and construction is.

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Q. Supposing you answer that yes or no—that is a question which can be answered by "yes" or "no". A. I will not. You have asked that question in a way which collects a whole lot of things. If I answer yes or no I must answer to them collectively. Now I am going to answer them as the facts dictate.

Mr. Farnsworth: If his honor is willing to go on for fifteen minutes——

The Court: I think you might answer either yes or no and then explain that, so that we will have some place to start with.

The Witness: I am perfectly willing to do that. 9788

Then, I answer the question, no; because I have pointed out that, as the term is very commonly used, the word, "impulse" has no reference whatever to operation. It refers merely to the existence of a particular construction of gap. For example, you will find some using the term "impulse" as entirely excluding the rotating gap—entirely excluding any gap except a particular type of construction of the multiple-plate-gap—excluding one which admits air, for instance, and yet there are splendid transmitters which have multiple-plates, all of them with air.

And so, when you are in a new art in this way you 9789
get terms used in all sorts of ways, without any definite meaning whatever, and the only way you can understand what a particular person means by that term, is to find out from him a full definition.

Otherwise then as a matter of construction, I would say there is no difference as a matter of principle whatever.

We have in both cases two circuits, one within which the energy is stored and the other to which it is transferred and which radiates it. They are characterized

9790 by the fact that one can radiate and the other cannot radiate. One can store a considerable—that is, relatively speaking—a large amount of energy. The other, relatively speaking, cannot; and they work by the transfer of energy from one to the other.

That, I understand, is the essence of all the present day transmitters which would come under the classification of Mr. Farnsworth's terms, and there is no difference in principle.

Q. As I understand you, you have this morning testified that the only difference between the two types of
9791 transmitters I have referred to, is one of degree of coupling. Is that correct—degree of coupling? A. No. It is not a matter of degree of coupling at all. You misunderstood me.

Q. What did you say, briefly, in respect to those two photographs which you produced, showing the different— A. (Continuing) If you have one gap in one condition and you want to get the proper, intended action of the Marconi apparatus in there with that gap, you must, for your best results, associate the circuits, or couple the circuits by a proper amount. Now that proper amount may be very much too tight for some other gap. It may be looser than is necessary for some third gap.
9792 It is merely a matter of adjustment. It corresponds to the tightening of the spring in a telegraph instrument. A particular construction and workmanship characterizing a gap enables the user or operator to couple it to obtain a good result.

What I said regarding the Simpson transmitter was that the constructor of this transmitter has given the set a little tighter coupling than the gap will continuously admit of. Therefore, when the gap is perfectly cold it gives a good operation, but as soon as it has been used a little and gets a little warm, then it begins to lose effectiveness to some extent by a retransfer of energy back to the primary circuit, as this photograph 53 shows.

Q. Do you understand that in practical use the defendant's Simpson mercury valve transmitter is not effective by virtue of that fact just stated by you? A. I have not said anything of that sort. The instrument when operating as shown in photograph 53 is operating in the same general way. It is losing some of its effectiveness. It is not as good, that is all.

Now, what it does, and what other samples of it do in regular practice, I do not know.

Q. Have you ever seen the Simpson mercury valve 9794
transmitter in service on ship board in ordinary commercial use by an operator? A. No.

Q. You, as I understand you, draw no distinction between an impulse transmitter and a transmitter of the Marconi patent type in suit, in respect of the relative number of oscillations in the spark gap circuit and in the antenna circuit, is that correct? A. That is correct; for the reason that it is, as the Bureau of Standards report (Deft's. Ex. No. 10) states, primarily a matter of the ratio of c over L. If that ratio is large, we have very few oscillations in the primary circuit.

That is characteristic of a number of the examples 9795
found in the Marconi patent.

If, on the other hand, that ratio of c over L is smaller, then there will be more oscillations in the primary circuit before the energy is transferred; and if it is made quite small, then there will be still more.

The Marconi patent sets forth both. There are examples, specific examples. One may be constructed, and which I have constructed, which illustrated both of those cases.

Mr. Taylor has testified in this case that the construction which was used for many years by the Marconi Com-

9796

pany in the ship stations and in its big station at Cape Cod, is the construction in which the ratio of c over L is large; that is, in which there was only a single loop of inductance and the wave length made up by the use of large capacity.

The form of the Marconi apparatus or patent, therefore, which was for many years quite generally known, was that having very few oscillations in its primary circuit.

Q. You say then, in short, that there is no difference, in your opinion, between the impulse transmitter and the
9797 Marconi patent in suit, in relation to the number of oscillations in the antenna circuit? A. There is no difference whatever, because they are the same thing; if the two are equally well adjusted.

Q. Then it would be a transmitter of the impulse type, in your opinion, if the oscillations in the primary circuit continued just as long as the oscillations in the antenna circuit? A. As long as there was no retransfer of energy the definition would, theoretically, apply. But, of course, that definition never exists in practice. I suppose different people would use terms in different ways. I do not think it is material, because the condition does not exist, and it is purely speculative.

9798

Q. What would there be "impulsive" about such a transmitter? A. The term has no descriptive significance.

Q. It does not mean anything, in other words, "impulse type"? A. Not in a descriptive way; absolutely not. It simply means that one operation of that condenser circuit—once getting rid of its energy, completes the process of transfer, and there is no re-transfer.

Q. Then you are quite certain that you yourself, as an expert witness in this case, did not make any distinction between the impulse excitation on one end of the Marconi transmitter of the patent in suit?

Mr. Betts: I think counsel should define, if the court please, what definition he has in mind when designating a transmitter as "impulse." The witness has very promptly said what his definition and understanding of that phrase is, and if counsel has another definition in his mind, it should be put into the question.

The Court: I rather thought that the witness gave the condition of his mind in answer to the inquiry.

Mr. Farnsworth: I wanted to make sure of that, and I want to be sure that he sticks to it. 9800

(Question repeated to witness.)

A. That depends on what the question means. That word "impulse" has been used in this case with so many different meanings, that I have not any doubt that I have, in following the particular witness that I was talking about, adopted his meaning. You may find that I have used it with half a dozen different meanings, because there are a half a dozen different meanings which have been given to it by the defense in this case. So that my usage and my understanding must be distinguished. I think in every case where I have used it as taking it from the defendant, as will appear from the definition. But 9801
my understanding of the term is not affected at all. You will find in the case that for a long period the term "impulse transmitter" has been used by the defendant's witnesses as implying one-half oscillation in the primary, and one-half oscillation only.

Now, a good many use it that way. Since, however, it has appeared that there were several oscillations in the primary of the Simpson mercury valve transmitter, the witnesses for the defendant have reversed their definition. Mr. Pickard, I believe, now understands that.

9802 F. N. Waterman—Recalled—Cross.

Mr. Skeel: Just a moment——

Q. (Mr. Farnsworth) Now, Mr. Waterman, notwithstanding everything you said this morning, I have no doubt that you agree with the definition of impulse excitation which is given by the standardizing committee of the Radio Institute as follows:

> "Excitation impulse. A method of producing free alternating currents in an exciting circuit in which the duration of the exciting currents is short compared with the duration of the excited current.
> 9803 (Note) A condition of short duration implies that there can be no appreciable reaction between the circuits."

You, of course, agree with that? A. Yes, I think that if you will give that definition due weight you will find it is absolutely what I said. That is to say, a primary fundamental condition is no reaction between the circuits. The explanation of the definition gives the foundation on which it is based. It is absolutely what I have said.

Q. Now, are all open circuits non-radiative—I mean closed circuits?

9804 Mr. Betts: Are you speaking of wireless transmitters or wireless receivers or wireless apparatus at all?

Q. (Mr. Farnsworth) Mr. Waterman, do all open circuits radiate? A. No open circuit radiates, if it is not properly excited for the purpose.

Mr. Farnsworth: Will you answer the question please?

A. I don't understand it.

 9805

Q. All right—we will pass it then. You are quite certain there are no practical advantages in an impulse transmitter over the transmitter of the Fig. 1, of the Marconi patent in suit? A. The transmitter in the Marconi patent is an impulse transmitter if it is constructed in accordance with the description of the Marconi patent, and properly operated.

Q. The transmitter of the Marconi patent is an impulse transmitter? A. If constructed according to the description of the patent and properly operated, yes.

Q. And that transmitter of the Marconi patent in suit
is unquestionably, you think, adapted to be supplied with 9806
an in-put current of 500 cycles a second, and be operative thereby? A. You cannot push a given structure beyond its capacity. I do not know what you mean now. You could not, with any apparatus known, have used 500 cycles ten years ago. It would not be a feasible thing. You could not have avoided the arcing. Ten years ago you could not have run an automobile at 50 miles an hour. There has been a continued improvement in the structure; there has been a continued improvement in the structure of wireless telegraphy and telegraphic apparatus, which has permitted the continued increase of frequency.

9807

Mr. Betts: Pardon me—you could not have avoided the arcing, where?

A. In the spark gap.

Q. (Mr. Farnsworth) Where? A. In the spark gap.

Q. Pray continue—I wanted to find out. A. What?

Q. Continue. A. Well, I have stated in my direct examination the whole substance of that. The spark gap in the wireless transmitter is the source of practically all the trouble you have with it. It is a nuisance. The best spark gap to-day is a nuisance—an awful nuisance. But

it is essential. It is the key to the whole operation. Now, the best efforts of the best men have been devoted ever since the Marconi invention, to improving the construction of the spark gap. You take a plain, open gap. You can operate that gap with a reasonable degree of success with a charging rate to your condenser of maybe fifty or sixty cycles per second. You can put an air-blast on it and you simply blow air across it. You may then do one of two things; you many increase the amount of power you put through it or you may increase the frequency with which you put the same amount of power
9809 across it. You may run it up to one hundred or two hundred, and you may keep it at a given rate and double your trouble.

Mr. Betts: When you say "rate", what do you mean?

A. I mean charges per second. I do not know whether I ever made that clear, your honor.

Mr. Farnsworth: I think not.

The Witness: (continuing) —how spark frequency is distinguished from oscillation frequency. The oscillation frequency of a wireless
9810 telegraph transmitter is fixed by the construction of the circuit. Referring to F.G.S.2 (see Vol. 2, p. 1080), there is a primary circuit containing condenser, inductance and spark gap. The oscillation frequency of that circuit is determined by the product of capacity and inductance. This capacity measured in proper units and this inductance measured in proper units gives the so-called oscillation constant. It fixes the rate of oscillation. Now, nothing that can be done to it will alter that, save an alteration of these constituent constants.

 9811

Mr. Hughes: Is that what they call the time period?

A. That determines the time period; it means, in the case of the Simpson mercury valve transmitter, working on the longest wave, that the time of one complete oscillation in that circuit is two one-millionths of a second, and that is fixed. Now, if we turn the switch that is on the panel board to another position, for instance to No. 4 position, then we have another time period. That is to say, the action of that switch is to cut
this condenser down until the time period is only one- 9812
half of what it was before. So that it then will take only one-millionth of a second for the complete oscillation.

But, now, it is evident that I might charge and discharge this condenser at twenty minutes past ten, I will say, and there would result the operation which I illustrated on F. N. W. 26 yesterday; there would be the oscillation in the primary, the transfer of energy to the antenna, the radiation of that energy. The system would have emptied itself. If I did not again charge the condenser, nothing else would happen.

Now, I may at ten twenty-five o'clock charge it again,
and at ten thirty charge it again, and there would be then 9813
twelve charges per hour and the spark frequency would be twelve per hour, but the oscillation frequency would still be at the rate of five hundred thousand per second if I had the longest wave; or a million per second if I had the shortest wave, in the oscillation of the energy in these circuits.

Now, usually, in practice, the number of times that we charge this circuit is from 120 to 1000 per second. The idea is the same as when I was using the hour—in using twelve per hour. It means that you radiate one wave train and then radiate another and then radiate another and the interval might be five minutes, as I supposed in my illustration, which, of course, would not be commercially useful. It might be one second; it might be the tenth of a second or the one-hundredth of a second or the one-thousandth of a second. Therefore, we have to determine in a wireless transmitter how many wave trains per second we are going to send out. And that was the frequency that I was referring to.

Now, it is evident that a large current oscillating in this primary circuit—referring to F. G. S. 2—will heat
9815 the spark gap to a certain extent. It must cool to a certain extent before another train of oscillations can be set up in it; because, otherwise, as I pointed out in my direct examination, the current will flow continuously from the power circuit, through that gap, and the wireless operation cease.

Now, a given construction of gap will cool at a given rate, and as I said before, you may increase that rate by blowing an air blast on it. You could not with a gap whose construction permitted it only to cool, we will say, at the rate of one hundred wave trains per second—you could not, with such a gap, undertake to operate the transmitter at a thousand per second. It would be quite im-
9816 possible. But if you could put air blasts enough on it, you could do it.

I have taken the oldest, crudest type of spark gap that ever was used in wireless telegraphy and operated it successfully and well at a thousand per second, but I used about one hundred and fifty pounds air pressure on the gap—in fact I am not sure but it was higher—to cool it, in other words.

Now, the attempt to get spark gap constructions which would cool fast enough has led to many individual constructions. And the difference between gaps is this difference of degree of heating, or rather of heat radiating —the ability to get rid of the heat by radiating it, and doing it quick enough—and it must be very quick indeed when we are going to charge, with these oscillation trains following one another at the rate of a thousand per second.

Now, the use of a thousand per second, or a large number per second, has a number of decided advantages. The amount of energy that we can put into a given condenser in a single charge is the capacity multiplied by the square of the charging voltage divided by two. The quantity
that we can put in every five minutes, on the assumption 9818
that we would charge once in five minutes, would be only this quantity every five minutes, or twelve times that quantity every hour—in a given time. In other words, we can put as much energy at a given voltage into a condenser as is expressed by C V squared, divided by 2, multiplied by N, where N is the number of times we charge.

In other words, if you were bailing water, you could bail a given quantity of water out of a boat at one pailful, and the amount you could bail per minute would depend on how many pailfuls you could take out; and the same way, the amount of energy you get into the wireless con-
denser at a given charge is fixed by its construction— 9819
just the same as the size of the bucket. Using the bucket as an illustration, the amount of energy we could get in per second, which determines the amount of use we get out, is the size of the bucket multiplied by the times we fill it.

Q. That means the number of sparks per second—the number of times? A. That means the number of spark discharges or oscillation trains.

Q. It is the number of times per second that the spark gap discharges? A. It is more correct to say the number

9820 of times per second the condenser is charged from the source, resulting, of course, in the sparks which give rise to the oscillation train.

Therefore, if we can get a spark gap which will cool so that we can charge it one thousand times per second, we can, with a given apparatus, get ten times as much out of it as we could if we only charged it one hundred times per second. Hence the motive that wireless telegraph engineers have had in increasing the spark gap is very evident—in increasing the spark frequency, I mean, is very evident.

9821 Q. Where, in the Marconi patent in suit, do you find any suggestion of the desirability of devising spark gaps which will permit ready cooling and, therefore, the advantages which you have just stated of high spark frequency? A. Well, if you mean specific reference, I do not find any. I have pointed out what the Marconi patent has to say.

Q. Where did you find in the Marconi patent any suggestion——

Mr. Betts: Let him finish his answer.

Q. (Mr. Farnsworth) Where do you find in the Marconi patent any suggestion of the advantages of impulse
9822 excitation charging?

Mr. Hughes: I don't think he should ask a new question until the old one is answered.

The Court: If he can combine the two—if not, he can disregard the last.

The Witness: The subject matters are rather difficult, your honor, to combine.

The Court: When I said "combine them", I mean to have one follow the other.

Mr. Farnsworth: I am only doing it, as I

3275

Defendant's Exhibit No. 54.

Fig 1

$N =$ Total Energy per second)

charges of C per sec

6

would do it with my own witness, to help him along, by interpolating a question—you can proceed with the long answer that you were on.

Mr. Betts: I think the witness should be allowed to finish his answer and then, if counsel wished to ask another question, they should do so, instead of interrupting and interpolating questions.

The Court: Oh, yes.

The Witness: I, therefore, point out that the total energy and the total work to be done, depends not merely upon the construction of the ap- 9830
paratus as determined by the fixing of its capacity and the fixing of its charge but by the number of times that we charge that per second; and that is the so-called spark frequency. When in my deposition I have spoken of one discharge per half cycle, or two discharges per half cycle, or multiple discharges per half cycle, what I have meant is this: the alternating current generator furnishes current which comes in waves, like that (illustrating) (reproduced opposite). If the apparatus is so adjusted that the highest frequency indicated at the point a given rise to a pressure across the gap which will just break it down, then, evidently, 9831
we can have only one discharge for this half wave of current, and that is called a half cycle, and when we speak of one discharge per half cycle, that is what it means. There will then be another at the point b——

Q. Another half cycle? A. (Continuing) The next half cycle, yes. But if the wave has a higher voltage the same gap would break down in the first half wave at a^1, and then after the discharge has taken place it might

FNH 27

Defendant's Exhibit No. 54.

Fig 1

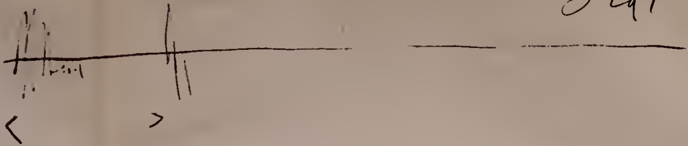
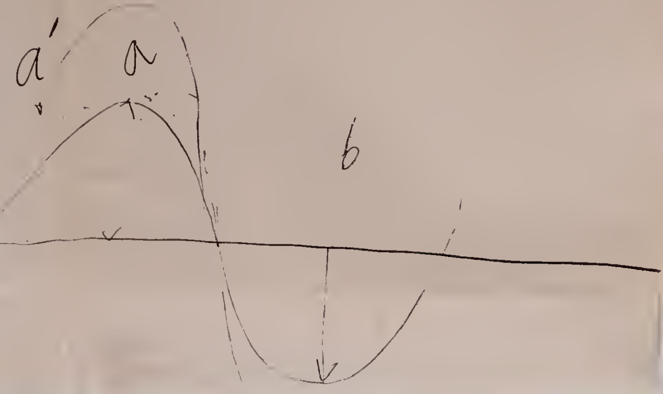


Fig 2 $\frac{CV^2}{2} \times N =$ Total Energy (per second)

$N =$ charges of C per sec

Fig 3



break down once, twice or three times more in that cycle. Then there would be two, three or four discharges per half cycle. So that if there occurred, let us say, 120 complete waves, or 240 half waves per second and there were one discharge per half cycle, then the spark frequency, the number of trains, would be 240—in this equation would be 240. But if the discharges took place at six or eight times per half cycle, then there would be six or eight times 240 to represent the number "N". Or that is—I have chosen for illustration the number which the Bureau of Standards report shows was the
9833 operation of the Simpson mercury valve transmitter as operated by Dr. Kolster at the Bureau of Standards, and the evidence of that is seen in his oscillogram attached to his report (Defendant'c Ex. No. 10), which is No. 3 (showing). The lines are quite faint, but looking at the lower line of curves, the court will see that the voltage starts to rise, as I just illustrated, and then the spark takes place and it falls, and then it builds up again, and by continuing you will see that there are an average of seven or eight of those per half cycle.

That shows how the Simpson mercury valve transmitter was operated by Dr. Kolster at the Bureau of Standards.

9834 The desirability, therefore, of increasing the number of charges per second, or of increasing N in this equation—is evident, to enable the same sized apparatus to give so much more power. The limiting condition was the ability of the gap to cool and, consequently, various gaps have been designed, and those most in use today are rotating gap, which puts, usually, two sparks in series and rotates the electrodes at high speed, and the so-called quenched gap, which uses very large plates of metal very close together—a lot of metal and very little gap so to speak. So that the plates have large radiat-

 9835

ing surfaces, and those are cooled by a low pressure draft of air over them, so as to aid in the cooling process.

So there has been an ingenious improvement in the details of construction of spark gaps; and you could not take the gap of a few years ago and use it with the frequencies of today. It would not stand it. And, just as in automobiles and everything else, there has been engineering development or improvement.

Mr. Betts: I would suggest that the drawing which the witness has just made be marked for identification as "F.N.W. 27", so that the record 9836
may be kept clear.

The Court: Very well.

Q. By the way, Mr. Waterman, you do not, I take it, by that first figure on F. N. W.—27 intend the court to understand that the commercially used wave trains of today, such as in the Simpson mercury valve transmitter, are of any such short length as indicated in that photograph? A. I do not intend that photograph——

Q. (Interrupting) Of course not. A. (Continuing) —to illustrate anything very precise. As a matter of fact, the significant part of the wave used in practice 9837
is not longer than that.

Q. By the way, how many oscillations are there in the Simpson mercury valve transmitter antenna, how many waves or radiated wave trains? A. That depends upon the ship you put it on.

Q. About how many in an ordinary ship? A. Well, there is a very great variation. It varies from a decrement probably of about .08 to a decrement of .2.

Q. Assume the Bureau of Standards decrement of .04. A. Now, the answer depends entirely upon how the matter is stated. We may state it on the basis of that number in which the first half of the energy is radiated. In that case——

Q. (Interrupting) Is that the usual way? A. (Continuing)—the decrement is .2.

Mr. Betts: Let him answer the question.

A. It is a common way. On that basis the number of waves required to radiate half of the energy is three, .2 being the decrement. However, if .08 is the decrement, then about six and a fraction would radiate one-half. We may express it on the basis of the current amplitude, or
9839 the antenna current amplitude falling to ten per cent. In that case we would have twelve and one-half in the case of the .2 decrement, or twenty-six, about, in the .08 decrement. Or, we may state it on the basis of the amplitude falling to one per cent, in which case we have twenty-four with .2 decrement, and fifty-seven for the .08 decrement. Or, we may state it on various other bases, for instance, when ninety-nine per cent of the energy has been radiated, in which case you would have eleven and one-half waves in one instance, and twenty-eight in another. The first few waves are the significant part. What happens after that does not count, because it is of
9840 no use.

Q. Now, will you be good enough to answer those two questions not yet answered?

The Court: Re-ask the questions.

Q. This question was asked at a certain very relevant point in your long answer this morning; that is, where do you find in the Marconi patent the suggestion of impulse excitation, or of construction of spark-gaps so designed, that is, so that they may be quickly cooled and permit of high frequency charge and discharge. A. I considered that in my direct examination, and I pointed

out that the Marconi patent describes structures and combinations, and does not indulge in theories of operation. We can only judge of the theories of operation from the performance of the structures, and the structures which the Marconi patent points out are such structures as are illustrated by the Simpson mercury valve transmitter, in that there is a primary circuit having the large ratio c over L, meaning a small number of primary oscillations for a preponderant number of specific illustrations. There are also illustrations in which the ratio c over L is much smaller, in which case there would be a larger number of primary oscillations, but 9842
in every case that I know of there will be less oscillations in the primary than there will be in the secondary.

Q. On the other hand, the place where the battery impulse excitation is described, is in the Lodge patent, prior to the Marconi patent. A. No, you are mistaken. Lodge had his theory of striking his antenna by lightning, as I have explained, based upon his studies in lightning, but the apparatus is inoperative in a commercial sense.

.Q Do you consider that the Simpson mercury valve transmitter is an infringement of the Lodge patent in suit? A. I certainly do, as to its use of this inductance. Since the Lodge invention, the Lodge discovery of the tremendous benefit that can be derived by putting a coil 9843
inductance into an antenna, it has been almost the universal practice to do it. Marconi shows it in the patent No. 763,772 in suit.

The Court: I think that answer is sufficient, Mr. Waterman.

A. The mercury valve transmitter has it at point L.

Q. Now, Mr. Waterman, I think you have said that some other form or construction of spark gaps is essential in order that we may use this high spark frequency, that is, several hundreds a second, for matters of practical importance. Can you explain to the court, or will you detail just why it is a matter of practical importance that the spark gaps shall be cooled in order that such high spark frequency may be used?

Mr. Hughes: Hasn't he explained that?

Mr. Farnsworth: I would not have asked the question if he had.

A. I have explained it so many times in both direct and cross that I hardly know what new explanation is
9845 wanted. I think I have made it clear that the spark gap is of necessity——

The Court: You think you have covered that subject? A. I think I have, yes.

A. I think I have, yes.

The Court: Proceed.

Q. That is all you can say about it? A. Why, no, I can lecture on spark gaps from now until noon——

Q. (Interrupting) Answer that question. A. (Continuing)—and not tell you half I know. His Honor told
9846 me not to continue.

Q. Referring to the Marconi patent in suit, the patentee states on page 2, gives a reference to his use of a persistent oscillator in the primary circuit. You understand that, I take it, to include a spark gap which will be so constructed as to be quickly cooled and prevent persistent oscillations in that circuit, that is your interpretation? A. I do not understand it to include a spark gap at all. The statement has no reference to a spark gap.

Q. What is your answer to my question? A. I think that is an answer to the question.

Q. That is, a persistent oscillator has nothing to do with the spark gap? A. Nothing whatever. It defines the form of a circuit.

Q. What does it refer to? A. It refers to the form of a circuit.

Q. Namely what? A. A circuit which is the closed form, and such as does not radiate.

Q. And irrespective of whether or not it contains a spark gap? A. No, not at all. It defines the characteristic circuit entirely without reference to the spark gap, no spark gap in it at all.

Q. It is persistent oscillator in that patent whether 9848
it oscillates one or a hundred times. A. Surely. so long as it consumes all its own energy, does not radiate.

Q. Then the expression ''persistent oscillator'' in the patent, you think, is intended to cover and include the case where the spark gap is so cooled as to prevent persistent oscillation in that circuit? A. I think persistent oscillator covers beyond any question any circuit which consumes its own energy exclusively, does not radiate.

Q. Can't you answer my question in the terms of number of oscillations, persistent oscillations? A. No, because I do not think there is any such circuit, that is, speaking of an uncoupled plain circuit acting.

Q. We are talking about Fig. 1 of the patent in suit, 9849
an operative Marconi transmitter, of course; are you talking about something else? A. Not, if you are talking about that description in the patent. because that description in the patent is——

Q. (Interrupting) I am talking about page , the first paragraph, commencing with line six, where he described his transmitter, which he says consists of two circuits associated together. one a persistent oscillator and the other a radiator, an open circuit radiator. A. I assumed you were.

9850 F. N. Waterman—Recalled—Cross.

Q. Yes. Are you talking about those two pieces and separating them and talking about them separately? A. I am talking about Marconi's definition of the structure of that circuit. He is not speaking about operation at all. He is trying to define a type of circuit, no matter how you arrange it. As long as you have that type you have it, and that type in the primary is the circuit which must oscillate until it has consumed its own energy. That is what "persistent" means. Oscillating energy will persist in the circuit because it cannot get out, and he contrasts that with the circuit that does not oscillate per-
9851 sistently, because the energy leaks out. It is the difference between a tight bucket and a leaky one.

Q. You think then that that Marconi transmitter of the patent in suit, as in Fig. 1, has a persistently oscillating primary circuit even if there be only a half oscillation in that primary? A. I do not mean any such thing.

Q. Suppose there is only half an oscillation in that primary, or two half oscillations, or three half oscillations, or four half oscillations, you think then that yet even so that is a persistently oscillating circuit. A. If that small number of oscillations is due to the consumption of energy in the circuit. May I explain what I mean there. because that really is a very important point?
9852
Q. Why certainly.

(Whereupon witness draws a diagram.)

The Court: You had better mark that "F. N. W.–28" (reproduced opposite).

A. I will do so. There is a circuit which will get rid of its energy under favorable conditions in about one and one-half oscillations. I do not call that a persistently oscillating circuit. The reason it gets rid of its energy is because it radiates it.

Defendant

FNH 28

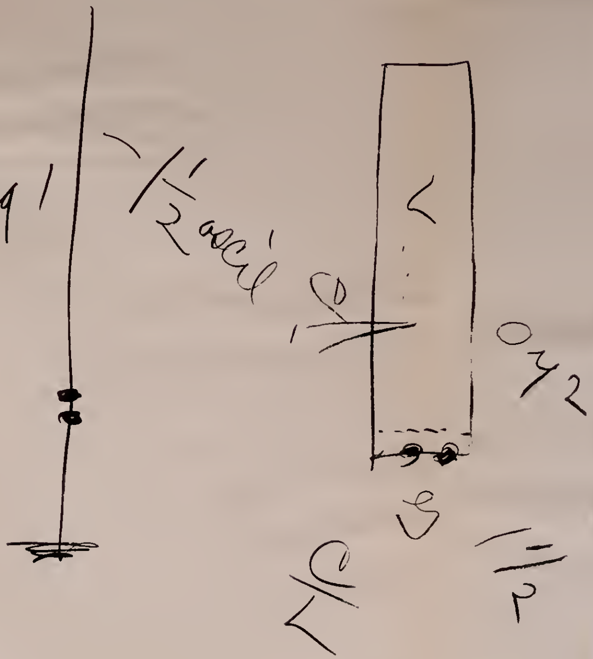
Fig 1

1/2 acid

Defendant's Exhibit No. 55.

FNN 28

Fig 1



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 9859

Mr. Betts: Mark that Figure 1 that you just drew.

A. (Continuing) There is a circuit, Fig. 2, capacity c, gap G, inductance just due to the wire bent around. First, I am going to consider that the spark gap is not there, that it is connected across as I have indicated by a dotted line. That circuit will consume its energy and probably could be made to consume its energy also in about one and one-half oscillations. Mr. Farnsworth's question would include those, both of those, and it is
obvious that I cannot answer questions like that. One 9860
of those circuits is a radiating circuit. It is the circuit that you cannot make have a large number of oscillations unless you change its construction. Here is a circuit in Fig. 2 which cannot radiate its energy and it will have a number of oscillations determined by its ratio of c over L, and by its resistance. Now, Marconi was trying to distinguish a circuit that will radiate from a circuit that could not radiate. The oscillations in Fig. 2 of F.N. W.-28 will persist until the energy is consumed. How long that will be depends upon how big a condenser you put in it, and how big an inductance. Marconi shows the biggest possible c and the smallest pos-
sible L for his construction in several of his illustrations 9861
but the character of that circuit is a circuit in which the oscillations must persist until they are consumed. It does not make any difference if c over L is large or small, that characteristic exists, and goes on forever. The particular number of oscillations depends upon the particular c over L.

Q. Suppose a condenser circuit has a very high resistance so that only very few, say three, oscillations take place in that circuit; would you call that circuit a persistent oscillator? A. Your question is not definite, because resistance alone does not determine——

9862 F. N. Waterman—Recalled—Cross.

Q. (Interrupting) You pass the question—if you cannot give an answer I will pass it every time. Don't you want to answer? A. I am glad to answer it. I don't think that is fair.

Q. Next question, if you are through I will ask the next question. A. I do not think that is fair dealing, Your Honor. I am perfectly glad to answer the questions, but I do not want to have to deliver a lecture every time.

The Court: Read the question.

(Question and answer repeated to the court.)

9863 Mr. Betts: Go into the rest of it.

The Court: Now answer the question if you are satisfied with the question.

The Witness: I can make my point clear in just a minute. If c over L has one value then the amount of resistance that would be required to bring about the reduction of the number of oscillations to 3, I believe the question said would be one thing. It might be so small you would have difficulty in getting big enough inductance, but if we have another c over L in this circuit then the resistance that it would require to bring
9864 about that condition might be very high, so high that you would have difficulty in getting it high enough. Now, the character of this circuit, whether it is to oscillate or not oscillate, has to be specified, for the reason that the so-called time constant of the circuit is composed of two components, one of which is c, and the other of which is R. You cannot define it until you define both. But the Marconi patent does give instructions which involve the use of large conductors, and those conductors stranded so that they will have the least high frequency resistance, hence, I

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would say that any circuit which had its number of oscillations reduced by deliberately putting in high resistance was at least a very bad illustration of what the patent intended to disclose.

Q. In your opinion, is the condenser circuit having as low as three oscillations a persistently oscillating circuit? A. I have answered that at least seventy-five times, I think. If that circuit is of the form shown in Fig. 2 of F.N.W.-28 then it is the form of circuit that Marconi called persistently oscillated, no matter whether in its operation in the transmitter it is three or thirty oscilla- 9866
tions.

Q. Then how, Mr. Waterman, do you reconcile that last answer with the statement in the Marconi patent in suit, on page 2, lines 12 to 20, as follows:

> "My experiments have demonstrated that the best results are obtained at the transmitting station when I use a persistent osccillator—an electrical circuit of such a character that if electromotive force is suddenly applied to it and the current then cut off electrical oscillations are set up in the circuit which persist or are maintained for a long time—in the primary circuit."

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A. I do not have to reconcile it. It is what it says.

Q. Well, also reconcile that statement with your testimony before His Honor Judge Veeder in the National case to the effect that the stored energy of the primary reservoir circuit is transferred to the elevated conductor or antenna at the rate at which that conductor can effectively raidate. A. I am very glad to have the opportunity of doing it again. I have done it a great many times. The Marconi patent describes various constructions. Some of those constructions involve a large ratio c over L, and some involve a small ratio. When I testified be-

fore in the National case before Judge Veeder the distinction, the fact that there could be a distinction based on whether that ratio was large or small had never occurred to me. The number of oscillations occurring in the primary circuit had never appealed to me to be of the slightest consequence, and it does not now. The defendant in that case had three forms of apparatus in issue; one using a plain, open gap, the old-fashioned unimproved device, one using the very up-to-date rotating gap, such as is used by the Arlington government station, and the other involving this multiple plate quenched
9869 gap, which the defendant here uses, only in a better, in my opinion, mechanical construction. The contention was made that the quenched gap operated very differently from the other, than the open gap, with the rotating gap coming somewhere in between, but there were very few oscillations of the primary circuit in one case and very many in the other. That occurred to me to be wholly a matter of degree. I could not see the slightest consequence to it, and I do not now. I gave a general description, educating Judge Veeder into some of the features of the art, and I used that language in that connection, without the slightest thought of differentiating one from the other. I would use it today in educating
9870 a person who had no grasp or conception of what all this wave business means, and I think it perfectly proper. May I have F.N.W.26? As I pointed out yesterday, in considering the Simpson mercury valve transmitter and its operation, as shown on the screen of the Braun tube, this Figure 2 shows oscillations occurring in the primary circuit. Figure 3 shows the building up of oscillations in the secondary circuit. I pointed out that this oscillation in the primary circuit, this big oscillation 1 in the primary circuit first started a small oscillation in the secondary circuit, and then not only maintained it but built

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it up, and again, in the third half oscillation, not only maintained but built up. Now, I said in that expression, "at the rate of". According to that it should not have built up, it should have stayed constant, that is all. That is an impossible physical condition, but Mr. Farnsworth has been talking about it ever since, and it was given in my fundamental, elementary, kindergarten explanation to Judge Veeder.

Q. Have you finished? A. Yes.

Q. You have said that you got a more powerful transmitter by using high spark frequency, and the currents
of arcing at the spark gap, as I understand you, is not 9872
consistent with the high spark frequency; why is it, what is the relation between arcing at the spark gap and this high spark frequency? A. I should say, if I understand your question, it was the rapidity of heating the gap. The question is not very clear.

Q. What has the heating of the gap got to do with arcing? A. Why, everything, everything.

Q. What? A. Well, the spark gap conducts by virtue of the so-called ionisation of the gases between. When it breaks down it breaks down because the gases become ionised. The passage of the spark disengages relatively little metal, but if the temperature gets high at the sur-
faces then the metallic vapors occur in considerable quan- 9873
tities in an ionised state, and when they occur in sufficient quantities then the current from the power source follows across the gap.

Q. Why do you want to stop the arcing in order to permit high spark frequency? A. You have to stop arcing to have any operation at all, Mr. Farnsworth.

Q. Why, explain it to the court, let the court see the facts, the reasons for all of this; don't force me to pull it out, just give the facts, please. A. It will take me one hour to give those facts. If you will tell me just what you want I will give it in two minutes.

9874 F. N. Waterman—Recalled—Cross.

Q. I want it. A. I do not see why I should be forced to lecture every time a simple matter is wanted. I do not know now and have not the slightest idea what it is Mr. Farnsworth wants, but I am going to talk until your honor stops me.

The Court: Well, just let me make this observation here: if there is anything, it seems to me, that you desire to have the witness emphasize or elucidate with reference to any proposition and can direct his attention to it, I wish it might be done.

Mr. Farnsworth: I will try, your Honor.

9875 The Court: Because we all know, as I thought during the course of this trial upon cross examination of one of the experts in response to Mr. Betts' inquiry, and I was impressed with it and thought about it a number of times, he told Mr. Betts he could talk indefinitely upon the proposition and still not say very much, and I have found that not only in this case but many cases that there is so much to say in relation to a particular thing by an expert that unless his attention has been directed to a particular thing, too much time is consumed.

9876 Q. You may confine your attention, Mr. Waterman, in answering that question, to these points, and their interrelation, namely, the matter of heating the spark gap terminals, the matter of arcing, and the matter of high spark frequencies, showing briefly why it is necessary in practice to have such construction and arrangement as that the spark gap surfaces shall be maintained cool in order to prevent arcing, and in order to permit the use of high spark frequency, which permit a much more powerful transmitter. You do not need to wander very far, just along those distinct lines. It is all one distinct matter. A. You cannot have an arc from a cold surface, because

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an arc, as I understand them, is a flow of current across an incandescent vapor. Now, if there is no heat there cannot be an incandescent vapor, therefore a cold surface does not support an arc. An arc is a good conductor. If we have an arc across the spark terminals s of F. G. S.-2, for example, we have a low resistance, and if that persists for long enough time one result must inevitably follow, and that is a flow of current from the power source through the spark gap back to the power source. Nothing can stop it. An arc is a phenomena of relatively long duration, because a spark has to gradually grow into an
arc, has to gradually heat up those terminals. The more 9878
vapor you get the lower the resistance, the lower the resistance the more vapor you get again, and so it keeps on building up. Now, the only reason this condenser can charge it all is because the spark gap s is open, as I have explained. But if the spark gap s is not open, if it is short-circuited by an arc you cannot charge your condenser, and if you cannot charge your condenser you have not got any wireless telegraph system. It is perfectly simple. Now, the physical fact is that you cannot have an arc on cold surfaces. As you must prevent your arc you must have cold enough surfaces. You get them perfectly well with any two pieces of metal opposed to one
another, so long as you do not heat them too fast. They 9879
will cool at a certain rate. If you do not heat them in excess of that rate you are all right. If a certain amount of energy can be put through them, fifty times a second, and have them keep cool enough to prevent arcing, why then you will be able to operate at that rate. But if that is just the rate and you try to discharge the condenser through them at a higher rate you cannot do it. The first thing that happens is a very irregular action of your gap, your condenser sometimes gets fully charged and sometimes it does not. Your gap really never goes out, but it

9880 F. N. Waterman—Recalled—Cross.

manages to maintain a sort of high average resistance so that you do not completely short-circuit your condenser, and as that goes on you get to the place where your condenser is completely short-circuited and you haven't got any wireless telegraph system. As I explained in my direct examination, you have to deal in wireless telegraph circuits with a seemingly impossible state of affairs, charging a condenser that is short-circuited, and it is only the fact that gap can be one thing one instant and another thing the next instant which enables you to do that. So that you are working under
9881 very critical conditions, and the problem of increasing the spark rate in order to get the increased output of a given structure has been a difficult one. Many minds have worked on it.

Mr. Hughes: For my own information, may I ask does it require a greater heat to vaporize a metallic surface to create arcing than it does to break down the air so as to permit the spark?

A. It requires no heat at all to break down the air. The air is punctured by extreme pressure on it, just as you can take a piece of glass and by simply putting a
9882 high pressure on it—no blow at all—you can cause that glass to completely fly to pieces. So by building up an electric pressure on the dielectric or air, whatever it is between the terminals of the gap, it, so to speak, seems to go to pieces, or, as the scientist says, the gas becomes ionised. That does not require heat, but to maintain an arc requires the metallic vapors, as I understand it.

Q. That is sufficiently clear as to the difference between an arc and a spark; do you want to add anything briefly to show the court the difference between an arc and a spark? A. Well, I have no objection to defining them to any length you want.

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Q. Very briefly the difference between an arc and a spark. A. An arc is a prolonged flow of current through a path of metallic vapors, so I understand it. A spark is the passage of a charge of electricity through a momentarily ionised gas. One is quick and all over with, and the other is a flow of power across the gap.

Q. Taking the Kolster Bureau of Standards report as to the antenna decrement .04 of the Simpson mercury valve transmitter, will you please make a diagram showing the oscillations in the spark circuit and the oscillations in the antenna circuit, in the usual way of showing
the antenna oscillations? A. The question implies that 9884
.04 is a decrement of the Simpson mercury valve transmitter. That is an absurdity, that is all.

Q. You fill in the data given in the Kolster Bureau of Standards report, whatever the conditions there show. A. May I have diagram F. G. S.-6? (See Vol. 2, p. 1113.)

A. F. G. S.-6 was produced by Mr. Simpson as showing the antenna of the Simpson mercury valve transmitter. It is the antenna of any particular wireless telegraph transmitter that happens to be put onto it. The defendant, in other words, installs a transmitter on a ship and it uses the antenna appliance there or puts up one
just as anybody else would put it up. The decrement of 9885
such an antenna may be .02.

Mr. Skeel: Just a moment, if the court please; this witness was asked to draw representations, a diagram showing the oscillations respectively in the antenna and in the spark circuit, based on the Kolster Bureau of Standards report which gave a certain specific decrement of .04. Now, the witness in answering that question is not doing that. He is is trying to show that the decrement is absurd.

9886 F. N. Waterman—Recalled—Cross.

Mr. Betts: If you will let the witness answer——

Mr. Skeel: He is not answering the question.

Mr. Farnsworth: He certainly has not commenced to draw his sketch.

Mr. Hughes: I do not understand that counsel can require a witness, without explanation or testimony, merely to draw a sketch.

The Court: I think that if the witness can make the sketch he should do it, and then if there is anything to explain in relation to it he can do that
9887 afterwards. I do not see that it is necessary to go over this in answer to that question.

The Witness: Your Honor is quite right. Mr. Skeel asked a question, but Mr. Farnsworth asked another question. Mr. Farnsworth never asks a question without giving a basis of false fact to it, and it is the basis of false fact that I have to reply to first.

Mr. Farnsworth: Thank you very much, Mr. Waterman.

The Court: Let us have the answer to the last question.

9888 Mr. Skeel: It is the answer to my question, it is intended to be the same thing.

Mr. Farnsworth: Make a diagram, like every one of those four Massachusetts witnesses did, showing, as near as you can, the fact.

Mr. Skeel: And based on the decrement given.

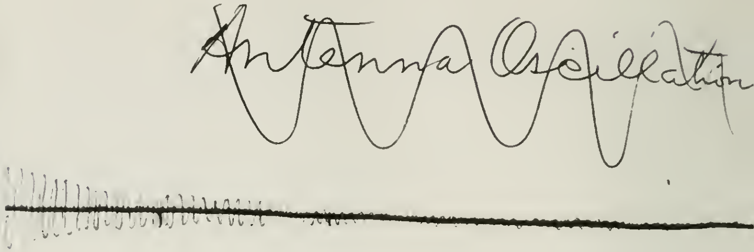
The Witness: May I answer your question?

Mr. Skeel: Answer both of them.

A. Mr. Kolster in his Bureau of Standards investigation appears to have operated a 2 kilowatt transformer at an output of 200 watts. (Deft's. Ex. No.

Defendant's Exhibit No. 56.

F.N.W 29



106 down to 1%

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10.) If he was putting it into its full power it was operating on an efficiency of ten per cent. He used an antenna of very small resistance and of very small capacity. Now, it is those two things that determine whether we are to have a large or small number of oscillations, before any certain given limit is reached. Mr. Kolster selected an antenna of the smallest capacity that a ship could be found to have, probably, and of a very small resistance. If I draw the number of oscillations that will occur I must myself select what basis I shall draw it on, it is not given to me
in the question. The number of oscillations is infinite al- 9896
ways irrespective of what the decrement is. It goes on forever. I am asked to draw the number of oscillations, and I am not told when to stop. Now, Your Honor can see that evidently I may start drawing here and draw until they are not visible to the naked eye any more, but I have got to keep on drawing until Mr. Farnsworth says "stop," and he has not yet said stop.

Mr. Skeel: We are not going to say "stop", Mr. Waterman, if that is the usual way of doing it you keep on doing it. Do it in the usual way.

A. There are various common ways. 9897

(Witness draws a diagram.)

I believe that is right. Referring to F.N.W.—29 (reproduced opposite), if I assume that the basis asked for was as Mr. Farnsworth suggested, that which he proposed to the witnesses at Boston, namely, a time when the height of this last oscillation has fallen to one per cent of the height of the first full oscillation. Of course I cannot draw rough hand oscillations of anything like proper form that are only one per cent of the height of the others unless I start with very large ones, but the antenna which Mr. Koltser selected for his Bureau of Standards result had a decrement of .04, as I remember the report. .04, unless I have made a mistake, would give a number of waves equal to 106 at the moment when the amplitude has become one per cent, and I tried to draw in this figure 106 oscillations of which the smallest is of the order of one per cent of the greatest.

Q. Now, Mr. Waterman, will you therefore indicate on that sketch the number 106 oscillations in that antenna? (Witness indicates.) The question, of course,
9899 Mr. Waterman, also, as you may recall, asked for the oscillations in the spark circuit beneath or above that one of the antenna.

Mr. Betts: May I ask whether the question was when the antenna was coupled or uncoupled to the primary circuit?

Mr. Farnsworth: We are talking, of course, as usual, about the apparatus of the machine hooked together in operative condition.

Mr. Hughes: That is what I understood to be the diagram.

Mr. Farnsworth: If I may finish my question,
9900 or rather, the repetition of part of the previous question that was not answered, I want, Mr. Waterman, to have you show the oscillations of the spark circuit on that same diagram in accordance with the Bureau of Standards report, that is two oscillations, I believe, as compared with the 106 you have shown in the antenna.

A. That is shown in the building up of the wave. I will be glad to draw it in another diagram.

Mr. Skeel: Yes, down below.

A. This wave first builds up. The energy, in other words, is——

Q. (Interrupting) I am asking you to draw the sketch.

A. (Continuing) —is in the primary circuit originally, and it builds up oscillation in the antenna circuit, and when it is all in the antenna circuit then decay by radiation occurs.

Mr. Betts: Call that Fig. 1, the one you have already drawn.

Mr. Farnsworth: No, the whole diagram is a diagram of one machine, and I do not want it 9902
divided into separate things. It is one figure, showing the action in the antenna and the action in the spark circuit. I want the sketch the way I want it without suggestion from our friend just now.

(Witness draws diagram) (page 3298).

A. This is very badly drawn. It is simply to get it onto my sheet that I have compressed the time axis——

The Court: So that we may understand, I think it would be well to mark this.

Mr. Farnsworth: I agree with Your Honor, 9903
but I do not want to make one of them Figure 2. I want the upper one marked "Antenna circuit", and the lower one "spark circuit".

The Court: It is immaterial to me. Pointing at that and saying that it shows so and so does not mean anything on the record.

Q. "Antenna circuit" above and "spark circuit" below, as I told you before.

The Court: Proceed.

9904 F. N. Waterman—Recalled—Cross.

Q. Antenna circuit oscillations is the upper one, and spark circuit oscillations is the lower one. A. If you want to tell me just exactly what I am to put on there and where I am to put it I will be very glad to describe it.

Mr. Skeel: Do you understand, he wants you to write "antenna circuit" over the upper figure——

Mr. Farnsworth: "Antenna circuit oscillations" and Below "Spark circuit oscillations".

A. Where do you want it put?

9905 Q. Will you do so please? A. Yes, surely, where?

Mr. Hughes: Mark it on the left hand margin there, up and down.

The Court: Just mark it anywhere so we will know.

(Witness indicates on diagram) (page 3298).

A. I have marked the upper figure "Antenna circuit oscillations on assumed decrement of .04 down to amplitude of 1%". What shall I mark the lower one?

Q. Mark it the same way. I have said mark it "Spark circuit oscillations", Mr. Waterman.

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The Court: Mark it whatever you intend it to be.

Q. Whatever the fact is. Let us have it identified. A. You have not been asking for facts, you have been asking about an assumed thing which is not the fact at all. If I am supposed to be drawing facts here, Your Honor, I want to understand it. I have drawn here what Mr. Farnsworth asked for. He asked me to draw two oscillations, and I have drawn two. That is not what the Bureau of Standards shows, the Bureau of Standards report shows that the number of oscillations in that circuit under certain conditions of operation is somewhere between 2.1 and 7.5. That is all it shows. I am not drawing facts. I am drawing what Mr. Farnsworth asked for.

Q. Use your own judgment based upon the facts shown in Kolster's Bureau of Standards report.

Mr. Hughes: That is a different question.

A. That is a very different question.

Mr. Skeel: It is intended to be the same one.

Mr. Hughes: That is not the proper way to examine him. 9908

A. Well, the tests made of the apparatus under similar conditions at the Cruft Laboratory showed that the actual number of oscillations in that circuit is probably about three and one-half or four. It is somewhere, a mean between the two impossible extremes that the Bureau of Standards report gives. Now, if I am to draw it that way I, of course, would simply double the number of oscillations. I have drawn it as I understand Mr. Farnsworth to ask for it, and it is entirely immaterial to me.

Mr. Hughes: Pointing to the lower diagram. 9909

A. Pointing to the lower diagram, yes. I have marked the lower diagram "Spark circuit oscillations assumed to be two in number."

Q. In your affidavit in the Buffalo case you stated, having reference to page 11 of the affidavit, having reference to Mr. Kolster's affidavit in that case: "This means that in the two cases the number of oscillations determined by the standard formula, based on a reduction of amplitude to 1% is ten and one-half complete oscillations. I have myself calculated this, and find that my result agrees with that calculated by Dr. Coffin.'' Were those calculations that you used at that time based on the logarithmic formula or the linear formula, linear decrement formula? A. I stated that it was based on the usual formula, and that is the logarithmic formula. I have never known the linear formula to be used before.

Q. Which do you now think is correct, the logarithmic formula or the linear formula, as applied to radio-telegraphy? A. Neither.

Q. Neither is correct? A. I never thought that either of them was correct. In fact, I know that neither of them
9911 is correct. The result must always lie between, but custom always sanctions the use of the logarithmic. Dr. Taege has only within a year or two years given us a formula by which we could do it on any other basis, and his work is most admirable. He gives us the other limit. We know that it cannot be logarithmic, because there is a varying resistance. On the other hand, we know it cannot be linear, because the apparatus is doing work, the energy would have to be all consumed in the spark gap which, of course, would make the apparatus useless. We know now, just as we have always known, that the true number of oscillations must lie between the linear and the logarithmic.

9912 Q. Well, do you or do you not now agree with the propriety of the use in measuring these radio circuits, of the use of the Taege or linear formula used by Mr. Kolster? A. Well, I think you misstate the facts. Mr. Kolster does not use that to determine the number of oscillations. He only used it to determine the limit. I approve of it, most certainly. I approve of every bit of information we can get.

Q. Is the Taege formula the correct formula to use in connection with radio circuits? A. No, absolutely not. It would be entirely without application in the case of the

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secondary circuit, and it is only applicable as to the primary circuit to determine one limit. Taege has given us a formula which is based on the assumption that all the energy is consumed in the spark gap. Well, that is very interesting, very valuable, but it is not practice, because all the energy is not consumed in the spark gap.

Q. Referring to your diagram F. N. W. 29, are there or are there not free oscillations in the antenna circuit? A. Surely. There are free oscillations in the antenna circuit. If we assumed this mode of operation that I have indicated in F. N. W.-29, and I have given the apparatus
the benefit of the doubt—I might have drawn any one of 9914
a number of other diagrams, but it would have taken time—I have assumed that the energy has been transferred by a series of oscillations in the primary circuit to the antenna circuit, and that thereafter the spark gap behaves in the ideal manner and lets the antenna circuit radiate that energy in accordance with the logarithmic law of decay—free oscillations, and, therefore, the oscillations become free after the spark gap current has become zero. In other words, I have assumed the half beat only.

Q. And in F. N. W.-29 is there or is there not shown any re-transfer of energy from the antenna back to the
spark gap circuit? A. No, there is no re-transfer of 9915
energy back to the spark gap circuit shown. If I had shown that the difference would have been that these lines which I drew for the envelope of the oscillations would have been like this (indicating), not a smooth line, but a wavy line. That is what would have been the difference.

Q. In your F. N. W.-29, as I understand you, your opinion is that the action shown in the lower part of the figure is that of a persistently oscillating circuit; that is, the action in the spark circuit is persistent relative to that long train of waves in the antenna in the upper part of the figure. A. Will you pardon me one moment—I have just noticed that what I drew in that lower figure as the building up process in the antenna and not the decay process in the primary, and I may have misled you.

Mr. Betts: Will you correct it during the noon hour?

A. Yes, but I was afraid I misled Mr. Farnsworth.

9917 Mr. Farnsworth: You certainly have. I asked you to put in that lower figure the action in the spark circuit.

A. Yes, but what I did was to copy what I had above, and of course it should be the same thing turned around.

Q. I would be very glad to have you make it right. What I wanted was a showing of the antenna in the upper figure and the spark circuit in the lower. A. I appreciate that, and I beg your pardon. It was simply carelessness.

Q. I will be very glad to give you the opportunity. A. It was a blunder on my part. I simply did not want Mr.
9918 Farnsworth to be laboring under a misapprehension.

(Whereupon the court takes a recess until 2:00 o'clock P. M.)

July 20, 1916, Afternoon Session, 2 o'clock. Continuation of proceedings pursuant to recess. All parties present as at former hearing.

F. N. Waterman, same witness, on the stand for further cross-examination.

Q. (Mr. Farnsworth) In circuit diagrams, Mr. Waterman, the arrow is employed, usually, to indicate adjustments employed by the operator in operating a telegraph set, is it not? A. That is a common convention at the present time, yes; as illustrated, for example, in exhibit 9920
68 (see Vol. 5, p. 3133) by the arrows 1 and 2, and the arrow 5 in F. G. S. 2.

Q. In plaintiff's exhibit 68, the diagram of the transmitter, you understand, do you, that those arrows 2 and 2a are, in fact, illustrative of adjustments which the telegraph operator on the ship uses in operating a transmitter? A. Oh, no; no, not at all. They are the adjustments that the installer makes use of. The operator at the present time is not allowed to make any adjustments whatever. If a set is provided with a wave changing switch, he may move that switch, but the switch itself has its connection so contrived that when the operator 9921
moves the switch he, unconsciously, does change those locations—such arrows as 1, 2 and 2a.

Q. The wave changing switch in the Simpson transmitter, shown in plaintiff's exhibit 68, in fact does change only the inductance coil g and the condenser c? A. Yes, that is right; that wave changing switch alters the secondary circuit by the changing of the antenna inductance g, being, in fact, the moving of the arrow 1 in exhibit 68, and it changes the time period of the primary circuit to a like extent by altering the capacity of the condenser c.

Q. And the wave changing switch of the Simpson transmitter does not operate to vary either of the adjustments at 2a and 2 on plaintiff's exhibit 68? A. No. Because to do that would alter the coupling too much. The condenser, being large and having very little effect on the coupling, the change is made there.

Q. Is there anything wrong or incorrect in Mr. Simpson's diagram F. G. S. 2 (Vol. 2, p. 435) of the Simpson transmitter? A. Wrong or what—what was the question?

Q. Wrong or incorrect? A. Why, I have not criticised it. It is good, to the extent that it shows details. Of
9923 course it does not show them all. It does not, for example, reveal the fact that the installer or tester who originally adjusts the apparatus must connect to a suitable point on this little scroll, or as Mr. Simpson terms it, "up-set," x. It does not show that the adjuster also moves the point of connection of the wire leading from that up-set to the small w, but Mr. Simpson explained those things in his testimony when he was considering the diagram.

Q. In the Simpson diagram F. G. S. 2, there is shown a horizontal antenna, a part of it, which is shown in full on the photograph F. G. S. 6 (Vol. 2, p. 477), isn't it? A. There is a diagrammatic indication at 2 of some sort of
9924 a capacity area at the top.

Q. Have you shown that horizontal ship's antenna, or indicated it on your diagram plaintiff's exhibit 68 of the Simpson transmitter? A. I have.

Q. Where? A. I have used the most commonly employed designation of it, which is simply a v placed at the top, and I have given it a letter, f. That is the commonest way of indicating it.

Q. You have not intended by that representation to make a representation which in appearance is that of the Fig. 1 transmitter antenna of the Marconi patent in

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suit? A. Why, yes and no. I assumed that the figure 1 of the Marconi patent in suit, by its hieroglyphic or convention, was illustrating any sort of a capacity area at the top, and the Simpson transmitter would be the Simpson transmitter with any sort of a capacity area at the top. I, therefore, intended my diagram to indicate any capacity area at the top of the elevated conductor.

Q. Referring to plaintiff's exhibit 68, this line s^1 and s^2, joining the condenser c and leading to the spark gap g; where do you get that line "s^1 and s^2" from, in respect to Mr. Simpson's diagram F. G. S. 2 and the circuit of his transmitter? A. That is the line 7 of Mr. 9926
Simpson's F. G. S. 2?

Q. Yes; but is that in all respects in plaintiff's exhibit 68 the same as F. G. S. 2? A. Surely, surely. I drew that figure before I had ever seen F. G. S. 2, that is all. That is an old drawing that I made for the Floridian case. It was made from a sketch by Mr. Weagant, as I remember it.

Q. The Floridian case? A. Yes; but I want to have it understood clearly that the showing in his drawings are identically the same.

Q. In plaintiff's Exhibit 68, in reference to the part at the left, the generator a and the induction coil and transformer c, how do those parts differ in the Simpson 9927
transmitter from the source and the induction coil in the Marconi patent in suit, Fig. 1, on the one hand, and from the source and induction coil in the Lodge patent, Fig. 4, transmitter on the other hand? A. I would like to have that question repeated.

Q. If there is anything in my questions which you do not understand I would be very glad to explain it to you.

(Question repeated to witness.)

A. Well, the differences, of course, structurally, are great. Functionally, in the broad sense, there is no difference—both are sources of current supply, that is all. I would be glad to go as much further as you like.

Q. Do you consider the mercury valve used in the Simpson transmitter an equivalent to the induction coil of the Marconi patent in suit, transmitter Fig. 1, in respect to uni-directional charging of the condenser? A. I would rather not use your word "equivalent," because I don't know just what you mean by it. I mean to say, that the induction coil has the effect in the wireless system of charging the condenser always in the same direc-
9929 tion, and the combination of transformer and mercury valve has the effect of charging the condenser always in one direction.

If that is what you call that "equivalency", why then, I assent to your question.

Q. In the Simpson transmitter is the energy charged directly into the antenna, or not? A. In any real sense, that is in the sense having to do with operativeness, it is not.

Q. Referring to an open circuited coil traversed by oscillating currents, does such an open circuit coil effectively or appreciably radiate energy?

9930 The Court: Let him answer it. I think it is pretty broad.

Q. (Mr. Farnsworth) To make it specific, take plaintiff's exhibit 68; take that coil g and strip everything else off it and leave nothing but the coil g there and suppose that coil supplied with oscillations; does that radiate appreciably? A. I do not know that of my own experience. I know that according to the public prints, aerials made up entirely of coils have been used for very small boats, like life boats, and I assume that it has, but I would not like to say that of my own knowledge.

Q. My question was; that coil g, stripped of everything else; no aerial and no nothing—would it then radiate if supplied with oscillating currents? A. That coil would, as I understand it, act as an aerial and radiate to an appreciable extent—to what extent I do not know.

Q. Would, or would not? A. It would.

Q. It would appreciably radiate energy? A. Appreciably, as compared to the same coil arranged in a closed circuit, yes.

Q. You referred in the Simpson transmitter to the matter of the ratio of large capacity to relatively small inductance. On what formula was your statement based 9932
that such a ratio would be that corresponding to the high decrement, and, therefore, a small number of oscillations? A. Why, the same formula that the Bureau of Standards report was referring to. I can derive it, or I can look it up in the book, either one.

Q. Well, I wish you would look it up and point it out to me in the book—I think you referred to the Fleming book. A. I can refer to any book. Its form is this: Decrement is equal to the square root of the capacity, divided by the inductance and multiplied by a constant—I have forgotten what that constant is—whether it is 2 phi or what it may be—I can work it out.

Q. No—I want the formula as it appears in the book? 9933
A. All right.

Q. I want a reference to it please. I think you referred to formulae or pictures or something in the Fleming book? A. I do not think that I have in that connection; but I will find it in the Fleming book I think. (Examines book).

Q. If you cannot find it at once, I will be glad to have you bring it in later in order to save time. A. Well, just as you please. I know it is here,—the difficulty is in finding it.

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Q. Suppose you bring it in later, and answer now whether, in your opinion, that formula applies to a circuit such as the converting trigger circuit of the Simpson mercury valve transmitter? A. Why, certainly, it does. It surely does. That is what the Bureau of Standards report says, and that is the universal acceptance, unless there is some very recent theory that upsets that, there cannot be any doubt about it.

Q. You will bring in that reference to the formula in the book later? A. Yes, surely.

Q. You have said that electrostatic coupling and di-
9935 rect coupling between two circuits are identical, I mean in general that was your statement. Can you state whether or not that also applies specifically to the case of two circuits where the capacity of the antenna and of the condenser are both initially and simultaneously charged?

If you have any difficulty in understanding that, of course, I will explain that what I mean is, the case of the Simpson mercury valve transmitter, where a part of the inductance 2 on plaintiff's exhibit 68 and the condenser c, or, as in F. G. S. 2, the condenser c and part of the inductance w, are in both circuits; that is, common to both circuits?

9936 A. Well, I can only answer that on two bases; first, the practical, and, second, the theoretical.

On the practical basis, the tests show that there is no substantial difference; that is to say, that it makes no difference whether the condenser is in both circuits or only in the primary circuit, as regarding the operation of the apparatus.

On the theoretical basis, the solution of the equations, I find rather difficult. I had Dr. Coffin go through the equations, and I followed them through, and the equations seemed to indicate that there is no difference; that

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is, no appreciable difference—the apparatus operates in the same way.

Q. As I understand you then, the operation is the same in the Simpson transmitter where the condenser c and part of the inductance w, F. G. S. 2, are common to both circuits—just the same in that case as in the case where the two circuits are coupled by the inductive, or two-coil transformer? A. Yes, of course, only allowing for entirely trifling distinctions.

Q. Now, Mr. Waterman, I understand you to say in your direct examination that the Marconi patent did not
define or specify in any way the nature of the spark 9938
gap of the transmitter Fig. 1 of the patent—if I make a misstatement you can correct me—that the patent, wherein it refers to the circuit containing the spark gap as a persistently oscillating circuit, thereby defines that spark gap as one which will permit persistent oscillations in that circuit and which will not quench the oscillations in that circuit. Do you agree with my statement?

> Mr. Betts: I object to the form of the question as argumentative, if the court please, and counsel is arguing his case in the questions and not making them definite and simple for the witness to answer.
>
> Mr. Farnsworth: I am trying to make my ques- 9939
> tions plain, so that Mr. Waterman will understand them.
>
> The Court: I will let him answer the question —note an exception.
>
> (Question repeated to witness.)

A. No, I do not. Because the statement not only does not refer to the spark gap, but it is not saying anything that has anything to do with the spark gap in one of the two alternatives rather than the other, which it sets forth.

9940 F. N. Waterman—Recalled—Cross.

If I may refer to a drawing which I made this morning, F. N. W. 28 (page 3285), there are two circuits—two types of circuits shown there. Both have a spark gap. These two circuits were existing, known circuits. It was known that the circuit of Fig. 1 is a circuit which exhausts its energy by the oscillation of the energy in the conductor, friction in the resistance and the conductor, friction or resistance loss in the spark gap, and by another and new and different form of energy dissipation, namely, by the radiation of waves.

The other is a circuit, the form of which is such that
9941 the energy stored in it will, when that circuit is taken by itself, oscillate until the energy is consumed in the friction in the conductors—resistance in the conductors which is analogous to friction—and the resistance in the spark gap. The conversion of the energy into heat, in other words. Now, taking those two circuits each by itself, each has the property which I have ascribed to it.

Now, those properties, of course, are radically and fundamentally different. The circuit of Fig. 1 of F. N. W. 28 has the power of sending out energy in waves. The Fig. 2 of F. N. W. 28 has no such power to any appreciable extent. The presence of the spark gap will shorten the number of oscillations in each, just as it would if
9942 there were a resistance in the circuit in each. But, for the comparative purpose that Marconi is talking about, it is not that matter of the losses in the spark gap that Marconi is talking about. He is talking about the fundamental properties of the circuits that distinguish them. The one, the power to radiate so that the energy goes off into space and does not remain in the conductor to oscillate until it is converted to heat. The other, the circuit Fig. 2, the circuit whose characteristic is that if it was disturbed, or the energy in it set in oscillation, there is nothing it could do except to oscillate until the energy is consumed.

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In other words, the oscillations persist to the point of their conversion into heat. In the other case, they do not; they go off into space. The energy is permanently shut out.

Now, that is what Marconi is talking about; and the presence of the spark gap is something that was common to both circuits in the art. In fact, at this date all of Marconi's wireless telegraph work was being done with the arrangement of Fig. 1 of F. N. W. 28, which had the spark gap in it.

The presence of the spark gap in the two circuits, therefore, has nothing to do with this quality that Mar- 9944
coni is talking about. He is talking about this radical property which distinguishes those circuits by virtue of their shape. That, to me, as an engineer, is what he means; and the one radiates its energy, as he says in this passage; the other cannot radiate its energy—its oscillations have to persist until they die out.

Now, he announced in the patent that he proposes to combine these; proposes to associate the type of circuit which cannot radiate but which can have a large condenser, and hence can store—with the circuit which can radiate and which, in order that it may be made to radiate, cannot have a large capacity, and hence cannot
store, in a large sense, and he associated those two cir- 9945
cuits, and he points out that that association becomes effective in proportion as you make those circuits in resonance with one another, and when the two circuits have their capacity and inductance so proportioned that their products in each circuit equal a like figure, then you have the best result in the transforming of the energy and you utilize the property of the closed circuit to hold energy—not dissipate it by radiation, but hold it in large measure and hand it over to the circuit which has not the capacity to hold it in large measure, but has the capacity to send it out into space.

9946 F. N. Waterman—Recalled—Cross.

Therefore, I disagree with the statement in the question, that this passage has anything to do with the spark gap, or determines the action of the spark gap in any way.

Its reference is not to something common to the two circuits, which might be used in either of them, but to something which differentiates and distinguishes the two circuits, and that one thing is the property of one to radiate; the inability of the other to radiate, but its ability to store.

Q. (Mr. Farnsworth) As I understand you, Mr.
9947 Waterman, on direct examination you are quite certain in your belief that the mercury valve in the Simpson transmitter has absolutely no effect whatsoever on the operation of the converting trigger circuit, is that correct? A. That is very strong language that your question uses. I do not know what it may mean. The Bureau of Standards report shows, and the tests at the Cruft Laboratory show that there is no effect which is appreciable in the operation of the apparatus as a wireless telegraph transmitter. That, I think, is clearly shown and the two agree in that respect. That is to say, so far as effective and useful operation of the wireless telegraph transmitter is concerned, you might as well take
9948 out the mercury value altogether.

Q. And, specifically, you are of the opinion that that mercury valve does not assist in the slightest degree in co-operation with the spark gaps, in giving the result of quenching of the oscillations in that trigger circuit? A. Well, "to the slightest degree" is, of course, a pretty strong statement. If you will change it to "an appreciable or significant or practically beneficial degree", I will say "yes."

Q. Referring to the matter of impulse excitation of the transmitting antenna, you stated in your affidavit in

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the Buffalo case as follows: "I agree that Lodge was the first to suggest the idea, but I understand that no one succeeded in carrying it out". Do you, therefore, disagree with Mr. Chaffee, in his deposition, that he considered that he had an impulse transmitter? A. (By the witness) No. I do not disagree with Dr. Chaffee at all. I disagree with the conclusions that the question seems to draw from Dr. Chaffee. What Dr. Chaffee does is the exact antithesis of what the Lodge patent sets forth. It is the most extreme opposite imaginable, in other words.

Q. (Mr. Farnsworth) You do not agree with Dr. Chaffee in the statement that his transmitter is an im- 9950
pulse transmitter? A. I do not remember that Dr. Chaffee made that statement. I understand Dr. Chaffee's apparatus, and I agree with everything that Dr. Chaffee said regarding it; but the apparent deductions which you draw from it are unfounded—completely unfounded on anything that he said, and I disagree most emphatically with those. If you will look at Dr. Chaffee's diagram of his apparatus you will see that there is no foundation at all for the deduction that I infer you draw.

Q. (Mr. Farnsworth) Assume two non-radiating, or what you call closed circuits, the two being alike in every way, save that one of the circuits has a single plain spark
gap, and the other circuit a number of quenched spark 9951
gaps of equivalent length; would there be any difference in the result of the number of oscillations in the respective circuits if the same amount of electromotive force is applied to each circuit and then cut off? Answer that yes or no if you can, please? A. I cannot answer it "yes" or "no", because there is no way of determining in which order I would insert the description. It is not sufficiently full, in other words.

I will assume that counsel contemplates and intends to describe two circuits exactly alike, having the same

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condenser and the same inductance, the same resistance, and differently constructed spark gaps.

I have made that test a good many times, using the same circuit, taking out the series of gaps and putting in the single gap, and I have never found any appreciable difference—that is, a constant difference. In the majority of cases I have found that the open gap, or the circuit when it had the open gap, had the fewer oscillations; but that is dependent, to some extent, on how much energy one puts in. I was using the amount of energy normally employed in the circuit which I experimented with for
9953 wireless telegraph purposes; and in that set of tests the circuit having the open gap, or the circuit when it had the open gap, usually had fewer oscillations than when it had the quenched gap; but that result was not invariable, and I should say, therefore, that there was no appreciable difference.

Q. Then, in your opinion, the multiple plate gaps do not act to quench at all? A. What is that question?

Q. Then, in your opinion, the multiple plate gaps do not act at all to quench the oscillation? A. I certainly did not say that. All gaps quench—it is only a matter of degree—if they did not, we could not have any wireless system.

9954 Q. Then several of these so-called quenched gaps in series do not quench any more effectively than a plain gap, such as the Marconi patent? A. I have not said that. On the contrary, I have very frequently explained that the quenched gap is an undoubted improvement. It would be quite ridiculous to say that the efforts of constructors in the years of the invention, the patent in suit, has been no use—has not made improvements. They have made improvements, very great improvements, and two of those are the two forms of so-called quenching gaps, which gaps are gaps that less resolutely maintain

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an arc. But I have only made one set of experiments—or rather, two sets of experiments, or possibly three, on that subject—and I have made a great many of them—and I can only give you those results in all good faith—that is what I have found to be the case. I do not say that it will be invariably the case, because I think you can overload a circuit, but I was using a circuit normally used in wireless telegraphy and I was using it with the normal load it has when it is ordinarily used.

Q. And what do most of the transmitters today employ; the old plain gap or series or quenched gaps? A. I have said before, and I think correctly, that the ma- 9956
jority of transmitters at the present time employ a series of short gaps to get a very much better cooling.

Q. Between extensive flat metal surfaces? A. Yes, or else the rotating electrode gaps. The very big stations mostly use the rotating electrode gaps; the little ones use both.

Q. Referring to these two circuits of a preceding question, will there be a greater number of resulting oscillations in either of those circuits under the conditions named in that question?

Mr. Hughes: You mean a greater number in one than in the other? 9957

A. (By the witness) Well, I have already defined what I understood the question to be, and I have already answered it to the best of my information and all the information that I have, in the negative.

Q. There would be no more oscillations in one case than the other? A. No.

Q. Why does the arc to-day employ the series of plates or quenched gaps? A. You were speaking of the circuits by themselves. When we couple the circuits to the antenna, one more certainly gives out at the time

when its energy has been transferred to the other circuit, than the other does. You see, the difference is this: As you ask the question you refer to two circuits just alike, save for the spark gaps. You put the energy into them. That energy is consumed within them. They oscillate until the energy is consumed. If the resistance is alike, the supposition is that the circuits are alike; and in my case; in my experiments I made it alike by using the same circuit. And your spark gap resistance does not differ very much. The time when the energy is being consumed is the time when the first oscillations are oc-
9959 curring; then it is being consumed most rapidly; so that subsequent changes of resistance do not become exact; hence the number of oscillations appears to be substantially identical in the two cases.

Of course, your question involved the proviso of an equivalent gap, and it is a little difficult to get a strictly equivalent gap; it requires a rather nice adjustment to be sure that you have an equivalent gap.

All my tests were made with the greatest care possible. Now, however, when you come to take these circuits and couple them to an aerial; you take theMarconi arrangement of associated circuits; you want to stop the oscillations at a particular time, determined, not only by
9960 the action of the gap, but by a condition of energy transferred. And it is difficult with the open gap to get that perfect regularity of action which will do that every time. It is much easier to get it if you rotate the terminals violently, or if you use the quenched construction, where you have a large number of plates, comparatively speaking; and, therefore, can presume a reasonably constant temperature of plates and condition of surface. So that the point being to end the oscillations when the energy has been transferred; when this receiver circuit has accomplished its function, you can do that with much greater

reliability by these newer and improved forms of gaps. The degree of effectiveness is very much greater. I would not want to be understood to deny that, for a moment.

Q. Have you corrected your diagram F. N. W. 29? A. Yes.

Q. Will you please now answer the question? A. What is the question, Mr. Farnsworth, will you restate it?

Q. No, I will ask the stenographer to read it.

(Here the stenographer hands typewritten copy of previous question asked the witness, to counsel for defendant.) 9962

Q. Now, Mr. Waterman, you were speaking of the figure which was formerly the lower one on F. N. W. 29—did you erase it? A. I just drew some lines across it—I did not think you wanted it erased.

Q. I will mark on F. N. W. 29, as I asked you to do this morning, the top portion of the figure as "Antenna oscillations", and I will mark on the bottom figure, as I asked you to do this morning, "Spark Circuit Oscillations", and I will repeat the question. A. All right. May I just make the suggestion; if Mr. Farnsworth is going to give the designation, I would like that he would take my name off the diagram. 9963

The Court: I understand that is a designation to fit his question.

The Witness: Well, I do not adopt it.

The Court: I might say that the designation of these diagrams does not conform to the witness' express idea, and his ideas are expresséd on the designation given to these diagrams. I think in the examination that we had better use the designation given by the witness. It otherwise might be confusing, while, perhaps, not to us, to someone else.

Mr. Farnsworth: I will have to find out what he meant then by what he put on here, in order that I may ask questions about it. I will erase the marking that I have put on it.

The Witness: Why not just call them Figures 1 and 2?

The Court: You may just designate them as Figures 1 and 2, and then refer to them in that way.

9965 Mr. Farnsworth: We have the same language at the side, where I did not see it. It says, "Antenna Oscillations" and "Spark Circuit Oscillations". I do not see why he objected to my marking the same thing on there.

Q. In your diagram, F. N. W. 29, as I understand you, your opinion is that the action shown in the lower part of the figure, which lower part is now marked "Fig. 2", that connection is that of a persistently oscillating circuit; that is, the connection in that spark circuit, which connection is shown by the bottom figure, is persistent, relative to that long drawn wave in the upper part of the
9966 figure, which is the connection in the antenna; is that correct?

Mr. Hughes: I object to that question as improper cross examination and as irrelevant, immiterial and incompetent. He is asking him if the one is persistent, relative to the other, and that is totally immaterial.

The Court: He may answer it—exception noted.

A. That is a question that it is almost impossible to answer, because it is based on a twisted use of terms

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in every instance. There is no authority for that use of terms. By just changing the use of words a little bit, Mr. Farnsworth makes it entirely impossible to answer, short of a long explanation.

Marconi, in the Marconi patent is talking, in this passage which Mr. Farnsworth refers to at the top of page 2, line 12, I believe it begins, about the results of his experiments to determine what the form of the circuit should be, and he says that his experiments show that the best results are obtained when he associates two circuits having different forms; one, the form such that
by itself it oscillated until the energy is consumed within 9968
it, because it cannot do anything else—it has not the radiating power. And the other, a circuit in which the energy does not oscillate until it is consumed in it, but is radiated out. Now, Mr. Farnsworth's question, if I answer it in any brief way, would make an entire departure from that. How many times a circuit oscillates depends upon its capacity and its resistance, and as it cannot radiate it depends upon nothing else. I might, for example, by my diagram Fig. 2 of F. N. W. 28, (page 3285) illustrate a circuit which will oscillate once or twice before its energy is dissipated, or which will oscillate thousands of times before its energy is dissipated
by that frictional process of heat in the conductors. 9969
Clearly, how fast that energy is dissipated will depend upon how much is dissipated per oscillation.

Now, the amount dissipated per oscillation will depend upon the mean current of that oscillation squared, multiplied by the resistances. If the capacity is large and the inductance small, the current will be large. The square of the current will be very large, and that, multiplied by the resistances will mean a rapid conversion to heat. But if the circuit is one which has a small capacity and has a large inductance, then the current which flows

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will be very small, because the conversion of the energy to the kinetic state will be involved through a large inductance; hence the amount of current required to store it will be very small; but if the current is small, its square is also small, relative to the square of a larger current; hence the energy taken out per oscillation will be less. Hence, starting with the given quantity of energy, there will be few oscillations in the first instance—many in the last, before the energy has all been transferred into heat in the conductor.

So that the circuit is the same. It has not been al-
9971 tered. It is not any more a persistently oscillating type of circuit in one case than it is in the other. And that is the radical mistake that lies at the foundation of Mr. Farnsworth's question. Marconi is talking about circuits which he fully describes. He describes, for the most part, circuits which have a very large ratio of capacity to inductance; therefore, circuits which do not oscillate a long time—it is impossible—but they are persistently oscillating circuits, because they are distinguished from the other kind of circuits in that the oscillations will persist in such circuits as Fig. 2 of F. N. W. 28 until their energy is entirely consumed in heat. You cannot get out of it. But the other type of circuit radiates that energy.

9972 Now, both types may have many oscillations or few. But many oscillations in the type Fig. 1 does not make it a persistent oscillator. It is a radiating circuit. Fewer oscillations in Fig. 2 do not make it a non-persistent oscillator in the way in which those terms are used to distinguish the sorts of circuits in the Marconi patent, if I understand it at all—and of course, I am only speaking as to my own understanding. The designation which the patent employs is all framed from the wireless telegraph point of view. It takes circuits which in two respects are opposite, from the wireless point of view. It com-

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bines those circuits; makes them co-operate, and because it does that we get its extreme effectiveness, as compared with what went before. Those two respects in which those circuits differ are, first: the Fig. 1 type has a mode of dissipating energy wholly apart from that the other has. It can throw it off into space and radiate it. The type Fig. 2 cannot, to any appreciable extent, exercise that function.

The second differentiation is that the circuit Fig. 1 cannot, of the given dimensions, store any considerable amount of energy; while the circuit, Fig. 2, can, of the given dimensions, store a very large quantity of energy 9974
—that is a matter of the size of the condenser.

Those are quite antithetical properties, and those are what Marconi is talking about, if I understand it at all, when he speaks of the combining of those two things to get a single and united result.

Q. Referring to the last question and to F. N. W. 29, to which it related, as I understand you, you cannot answer the question by yes or no? A. Well, I think it would surely be misleading one way or the other if I did. I think I have explained the point that it does not make any difference how many oscillations there are. The property of the circuit is what Marconi was talking about and what we are talking about. 9975

Q. Did you ever examine the Braun tube equipment of the University of Washington? A. Yes.

Mr. Skeel: May I ask one question.

Q. (Mr. Skeel) Mr. Waterman, to make this concrete, is this a fair statement of your position: That the term "persistent oscillator" as used in the patent, is merely descriptive of the form or shape of the circuit, and does not mean a circuit in which oscillations are, in fact, maintained for a long time; is that a fair treatment? A. No;

9976 F. N. Waterman—Recalled—Redirect.

I do not think so, Mr. Skeel, for this one reason; that language is not descriptive; it is designative. It is a distinction between circuits of different properties with respect to radiation. Otherwise I would assent to your proposition.

Mr. Skeel: That is all.

REDIRECT EXAMINATION.

Q. (Mr. Betts) Mr. Waterman, referring to your drawing F. N. W. 29, (page 3298) upon what formula
9977 did you calculate, or what formula did you use in drawing Fig. 1—logarithmic, linear, Taege, or what? A. The logarithmic formula I used in determining the number of oscillations that would occur, assuming the antenna to be oscillating freely and to have the decrement that I was asked to assume.

Q. And what formula did you use in drawing Fig. 2 of F. N. W. 29, under the conditions assumed in the question? A. I did not use any. I was asked to draw a diagram for two oscillations, and I did so.

Q. Is it your understanding that F. N. W. 29, either Fig. 1 or Fig. 2 or both of them, is representative of the
9978 condition when the spark gap circuit is coupled to the antenna circuit? A. No. No, it is not; for the reason I have explained. I will state briefly——

A. I will state briefly. Namely, that the two oscillations to which Mr. Farnsworth referred I assume are the 2.1 oscillations which are given on page 7 of the Bureau of Standards report, and, as that report makes clear, those are based upon the Taege formula. That formula is one which assumes that the resistance is wholly in the spark gap, which of course it is not.

The other formula which the Bureau of Standards report gave is the logarithmic, and the report makes it

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perfectly clear that neither one of those applies to a circuit which has both constant resistance and variable resistance. The distinction between the logarithmic formula and the Taege formula or linear formula, is that the logarithmic formula presumes that the entire resistance encountered by the current does not change. The Taege formula, or linear formula presumes that the entire resistance of the circuit does change, and changes in a way that is inversely proportional to the current.

Now, neither of those things are true. The circuit comprises a certain constant resistance; that of its conductor; it comprises a certain constant resistance, that of 9980
the pushing of energy over into the other circuit—speaking of the primary circuit—it consists of a certain variable resistance which is in the spark gap. The effect, therefore, is of a resistance varying, but not varying as Taege assumed, in an inverse manner to the current. Hence, the total number of oscillations which must come in the circuit is somewhere between the one limit and the other.

Now, if we assume that the logarithmic limit is correctly determined, then the logarithmic would represent the maximum, and the linear the minimum number of possible oscillations—the actual number lying somewhere in between. But, as a matter of fact, the formula 9981
by which we determine the logarithmic decrement does not apply, as I understand the authorities on the subject, when we get much above a decrement of two-tenths; hence the determination made by the Bureau of Standards is merely that which is customarily made for lack of an accurate method, and it does not tell us how many there are; but we may presume that the actual number is between the lower and the upper limit; the fact being that if there is an error it will move nearer to the upper limit and not nearer to the lower limit, because at the

rise of the decrement, if I understand the theory and the books, the tendency of the method of measurement used is to overestimate the decrement, and, therefore, underestimate the number of oscillations.

If I had drawn F. N. W. 29, for example, as I am led to believe that it should be, from the tests that have been made of the Bureau of Standards report, I would have shown it as four oscillations, in Fig. 2.

Q. Mr. Waterman, reference has been made on your cross examination to the passage in the Marconi patent on page 2; I will ask you to read the whole of the para-
9983 graph, beginning on page 2, line 6 to line 24, inclusive, and state whether or not to you, as an engineer, that is descriptive of the two circuits coupled together—in a condition of being associated or coupled together? A. I have read the passage, and after re-reading it—I have read it many times—it does not alter my understanding of it. It is, as I take it, a designation of the two circuits which are to be taken and which are to be put together. It refers to the circuits by themselves, and subsequently describes the putting together of those to make the transmitter of the patent.

Q. Something was also said in your cross examination with regard to the equivalency of using a two-coil or
9984 auto-transformer, instead of a condenser common to both the antenna and spark gap circuit. I believe you stated that auto or single coil transformers have been used—are those also equivalent instrumentalities in your opinion? A. I understand that they are exact equivalents, and I think that that is agreed in this case. I read Mr. Pickard's statement to that effect, and I believe at the present time it is always accepted.

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RECROSS EXAMINATION.

Q. (Mr. Farnsworth.) Is it not a fact, Mr. Waterman, that Mr. Kolster's Bureau of Standards report as to the number of oscillations in the converting trigger circuit of the Simpson mercury valve transmitter, was shown to be substantially correct by the Chaffee, Massachusetts tests? A. Yes, decidedly. I think they agree remarkably well. All we have to do is to understand them both rightly to get exactly the same meaning.

(Witness excused.)

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(The following testimony of Mr. Waterman was introduced shortly afterwards in the proceedings and by direction of the court is inserted at this point.)

Q. (Mr. Farnsworth.) Will you please give the books and the page numbers of those formulæ and the formulæ themselves that you were to produce? A. Yes. I found the formula to which I referred in the book by Dr. Zenneck, the English edition.

Q. Which book of Dr. Zenneck, the large or small? A. 1915. There is only one in English that I know of, the book entitled "Wireless Telegraphy."

Q. A small book? A. A small book, yes, 1915, page 15, as follows: 9987

> "Decrement equals pi R times the square root of c over L."

I find the same formula in the Fleming book, "Principles of Electric Wave Telegraphy," page 214, this being the 2nd edition. It is the same formula except that it is a formula for the decrement for per half oscillation, instead of the decrement for whole oscillations, that being the usual English way of stating it. Therefore, the decrement is given as one-half that which I have just

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given, and of course, gives identically the same result when we take the decrement for a whole oscillation. This shows what I stated, that the decrement depends upon the capacity and upon the resistance and that, therefore, the number of oscillations which depends strictly upon the decrement depends also upon the capacity and upon the resistance.

Q. Did you give the page in the Fleming book also? A. Yes, page 214.

(Witness excused.)

9989 Mr. Betts: Mr. Farnsworth, are you going to offer F. N. W.—28 and F. N. W.—29, which were made in cross examination?

Mr. Farnsworth: Yes.

Mr. Betts: As Defendant's exhibits?

Mr. Farnsworth: Yes.

The Court: Let them be marked.

(Drawings marked "F. N. W.—27," "F. N. W.—28" and "F. N. W.—29," received in evidence and marked Defendant's exhibits Nos. 54, 55 and 56, respectively.)

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Roy A. Weagant, recalled as a witness on behalf of Plaintiff, testified as follows:

Q. (Mr. Betts) You are the Roy A. Weagant who has heretofore testified in this case? A. I am.

Q. Have you since the court adjourned in April examined a sample Simpson mercury valve transmitter and made any tests on it? A. Yes, I have.

Q. Whereabouts? A. At the laboratories of the Marconi Company in Aldine, New Jersey.

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Q. Was that prior to or subsequent to the time that this transmitter was sent to the Cruft High Tension Laboratory of Harvard University? A. It was prior to that time.

Q. From your examination of the Simpson mercury valve transmitter will you state how many oscillatory circuits there are in it? A. There are two oscillatory circuits in the Simpson mercury valve transmitter: the spark gap or closed oscillating circuit, and the antenna or open oscillating circuit.

Q. You have just spoken of the spark gap circuit as being a closed circuit, is that the terminology used among 9992 radio engineers descriptive of such a circuit as that in the Simpson mercury valve transmitter? A. That is the conventional way of referring to a circuit of that type.

Q. And you have just referred to the fact, or spoken of the antenna circuit as an open circuit; is that the terminology employed among radio engineers as descriptive of such a circuit? A. That is the ordinary terminology employed as descriptive of that circuit.

Q. Did you visit the Cruft High Tension Laboratory of Harvard University in July of the present year? A. Yes, I did.

Q. When did you go there? A. July 1st.

Q. And you remained how long? A. Until the close 9993 of July 5th.

Q. Had you ever been there before? A. No, I never had.

Q. You were present when tests were made on the Simpson mercury valve transmitter by Dr. Chaffee on July 3 and 4, when representatives of the defendant were present? A. Yes, I was present.

Q. What had you to do with suggesting the method or character of any tests conducted by Dr. Chaffee on July 3 and 4? A. Nothing at all that I remember.

9994 Roy A. Weagant—Recalled—Direct.

Q. What had you to do with the conduct of the tests made by Dr. Chaffee on July 3 and 4 on the Simpson mercury valve transmitter? A. Nothing, except that I assisted Dr. Chaffee by closing switches and turning rheostat handles as he directed me to do.

Q. On that occasion did you examine the connections of the Simpson mercury valve transmitter? A. Yes, I did.

Q. How did the connections of the circuit of the Simpson mercury valve transmitter agree with those shown in Chaffee diagram No. 1, which I now show you? A.
9995 Well, I think Chaffee diagram No. 1 is a perfectly correct representation of the circuits of this transmitter as I traced them out. I have seen this particular diagram before and checked it more closely than I could in an offhand way here.

Q. Have you heard or have you read Mr. Simpson's description explanatory of the way he adjusts the Simpson mercury valve transmitter spark gap circuit to what he calls the nodal point on the antenna circuit? A. Yes, I both heard his testimony and have since read it on that point.

Q. Did you see Dr. Chaffee adjust the Simpson mercury valve transmitter circuit prior to the tests of July
9996 3 and 4? A. Yes, I saw him make a good many adjustments.

Q. If the method described by Mr. Simpson did in fact locate the connection of the spark gap circuit to the antenna circuit at a point of potential in the Simpson mercury valve transmitter, what can you say as to whether or not Dr. Chaffee adjusted the Simpson mercury valve transmitter to that point during the test of July 3 and 4, as you saw them? A. Well, Dr. Chaffee did exactly what Mr. Simpson's instructions called for in adjusting the point of contact of the spark gap circuit to the antenna circuit.

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Q. And did you observe Dr. Chaffee's connection of the Braun tube apparatus to the Simpson mercury valve transmitter when it was tested on July 3 and 4? A. Yes, I observed it very carefully.

Q. I will ask you, as a constructor and designer of wireless telegraph apparatus, to say whether or not Dr. Chaffee's connections of the Braun tube apparatus disturbed the operation of the transmitter in any way? A. No, it did not. In fact, I noticed several times the operation of the apparatus with the measuring circuits attached and with them off, and there was no difference whatever in the operation of the set. 9998

Q. Did any of the representatives of the defendant present on July 3 or 4 make any observations with respect to the correctness or incorrectness of Dr. Chaffee's connection of the spark gap circuit to the antenna circuit? A. No, they did not, as far as I know.

Q. I will ask you by referring to Chaffee diagram No. 1 whether you, as a constructor and designer of wireless telegraph apparatus, can state whether the current coil e and leads to the Braun tube were such as to accurately shunt a constant portion of primary current? A. Yes, they were so connected as to shunt a constant portion of the current flowing in the spark gap or closed oscillating circuit. 9999

Q. I will ask you whether, as one skilled in the construction and design of wireless telegraph apparatus and the use thereof, whether Dr. Chaffee's adjustments were correct to secure the best results as to note and output of the Simpson mercury valve transmitter? A. They were, yes, the best that could be made.

Q. Did you see Dr. Chaffee adjust the apparatus to secure the best result of which the transmitter was capable of securing? A. Yes, I did.

Q. Was the set operating during those tests in such a

10000 way as would be commercially useful, assuming, of course, that a regular antenna was substituted for a dummy antenna, as used in the test? A. Yes, it was operating in a way which would have been commercially useful, and which would have enabled one to have telegraphed, sent messages to a distance in a perfectly satisfactory manner.

Q. You have stated, I believe, that prior to going to the Cruft High Tension Laboratory on July 1st, that you had tested the Simpson mercury transmitter; was that with a 500 cycle machine? A. I tested it with both the 500 cycle machine and the 120 cycle machine.

10001 Q. How did the note compare when the set was operating during Dr. Chaffee's tests on July 3 and 4 with the note of the set as you had previously operated it prior to July 1st on the 500 cycle machine? A. It was substantially the same.

Q. What is your opinion, from such tests as you have made with the Simpson mercury valve transmitter, as to the character of the spark gap circuit; that is to say, the oscillatory character? A. Why, the spark gap circuit of the Simpson mercury valve transmitter is an ordinary oscillating circuit. I cannot see that it differs in any respect from hundreds of others with which I am familiar.

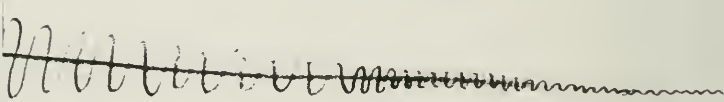
10002 Q. How many oscillations are there in the spark gap circuit of the Simpson mercury valve transmitter—I ask this question based upon your own tests, and also upon tests which you saw Dr. Chaffee make at the Cruft High Tension Laboratory? A. Well, there are at least two complete oscillations, and probably one or two more. The only absolutely definite thing is that there are two.

Q. Now, will you make a diagram illustrating in a convential way the two complete oscillations which you say exist in the spark gap circuit of the Simpson mer-

Phil 4

Fig 1

Fig 2



ntenna Circuit Oscillation

Spark Circuit Oscillation

cury valve transmitter? A. (Witness draws diagram) The diagram (Reproduced opposite) which I have drawn shows two and one-half complete oscillations. The oscillation of the primary circuit of necessity starts at a maximum value and it must of necessity stop at the zero line. Therefore, I show two and one-half instead of two oscillations as if the visible evidence of the photograph plates were on the screen, which showed two. It is perfectly certain that there are two and one-half oscillations existing there.

The Court: You had better mark that the next number. 10010

Mr. Betts: Mark that R. A. W.—4.

(Witness marks diagram).

Mr. Betts: I offer this diagram in evidence as plaintiff's exhibit No. 71.

The Court: It may be admitted.

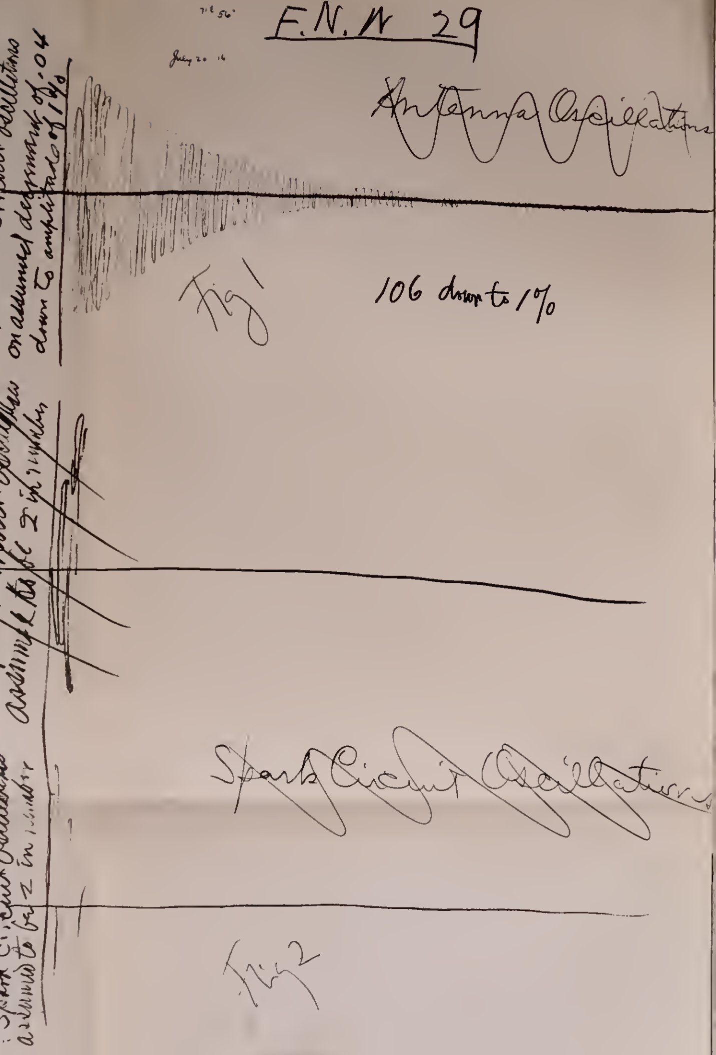
(Diagram marked "R. A. W.—4" received in evidence and marked "Plaintiff's Exhibit No. 71.")

Q. On the occasion of your visit to the Cruft High Tension Laboratory when Dr. Chaffee conducted tests on the Simpson mercury valve transmitter, did you personally see two or two and one-half oscillations on the screen of the Braun tube? A. Yes, I did. 10011

Q. That is, while you were there between the 1st and 5th of July? A. Yes.

Q. And what, in your judgment as a designer, user and constructor of wireless telegraph apparatus were shown by Dr. Chaffee's tests, and also what have your own tests shown on the Simpson mercury valve transmitter relative to the transfer of energy from the spark gap circuit to the antenna circuit? A. Well, both the tests which Dr. Chaffee made and observations which I made of those tests showed very conclusively that the

Defendant's Exhibit No. 56.



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energy was transferred from the closed or spark gap circuit to the antenna circuit, and that this transfer was not a thing taking place instantaneously, but required the two and one-half or three complete oscillations for all of the energy to be transferred from the spark gap circuit to the antenna circuit. That was shown in the photographs which were made of the current in the antenna circuit. It was also shown in the photographs which were made of the current in the primary circuit. In the latter case the primary circuit oscillations died down, showing that the energy took a little time to get
10013 out of that circuit, whereas, the oscillations in the antenna circuit slowly built up, and then slowly decayed away again, showing that at first the antenna circuit had no energy, and that after the lapse of a little time, two or three complete oscillations, it had received the energy from the spark gap circuit. Then at a time subsequent to that it was radiating that energy away in the form of electro-magnetic waves, and the waste or losses in the circuit. So that it went through the complete cycle of events, first of all receiving the energy, and immediately following that the business of getting rid of that energy.

Q. Did you make any tests of the Simpson mercury
10014 valve transmitter to determine whether or not the set worked any differently if the condenser C, as it has been called in this case, was in the spark gap circuit alone, instead of being common to the spark gap circuit and the antenna circuit? A. Yes, I made that test, and I witnessed Dr. Chaffee making the same test, and the difference in the operation of the set was so small that it was essentially negligible. It required a slight readjustment to get it in accurate tune again, but it might have been left without even that slight readjustment and for all practical purposes performed equally as well.

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Q. Did you make any tests or observe any tests made by Dr. Chaffee as to what effect the presence of the mercury valve has on the wireless circuits of the Simpson mercury valve transmitter in operation? A. Yes, I made the test myself, and I saw Dr. Chaffee make it. The mercury valve was short-circuited, that is, it was so connected that it ceased to operate. The transmitter, however, as a whole went on working substantially in the same way. It would have been very difficult for anyone who had not seen the change made to know that the mercury valve had been taken out.

Q. From your experience as a designer, constructor 10016
and user of wireless telegraph transmitters, is it correct to say that the antenna of the Simpson mercury valve transmitter is directly charged? A. No, it is not correct.

Q. Have you yourself tested the Simpson mercury valve transmitter or seen any tests or observations made by Dr. Chaffee in your presence to determine whether or not two circuits, the spark gap circuit and the antenna circuit, were in any way, to any degree, in a condition of resonance? A. Yes, those circuits were measured by Dr. Chaffee and I witnessed the measurement and results, and those measurements showed that the spark gap circuit and the antenna circuit were as nearly exactly in tune 10017
as is possible to measure it.

Q. Now, will you please explain to the court why you say that it is not proper or correct to describe the Simpson mercury valve transmitter antenna as directly charged? A. Well, that has been shown very clearly, of course, in the photographs about which various witnesses have testified, but there is another way in which that was shown very clearly during the tests made by Dr. Chaffee. We used for an artificial antenna a couple of ordinary Leyden jar condensers, and during the tests several peo-

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ple noted that there was very considerable brush on those condensers. Well, as a matter of fact, that brush indicated that each condenser had on it at least 20,000 or 25,000 volts, so that with the two condensers in series, as they were connected, this meant a total of 40,000 or 50,000 volts. Now, if the antenna were charged directly the only voltage which it could receive would be the voltage across the condenser, which has been called c, the large condenser. Now, the voltage across this condenser is something of the order of 3,000 or 4,000 volts, consequently, if the antenna had been charged directly
10019 and had had this 3,000 or 4,000 volts then these other condensers could not have given evidence of having on them voltage of the order of 40,000 or 50,000 volts, and the only way in which that voltage could rise to such a value is in the way that the photographs have shown, namely, by the transfer of energy from the spark gap circuit to the antenna circuit, and the gradual—well, rather fairly rapid—building up of this voltage.

Q. How is this building up of oscillations accomplished? A. Well, it is accomplished by the oscillations in the spark gap circuit which last long enough to transfer to the antenna circuit all of the energy in the spark gap circuit.

10020 Q. If there had been, as a matter of fact, only one-half an oscillation in the primary circuit of the Simpson mercury valve transmitter how would that have manifested itself in the antenna circuit with regard to this potential that you mention? A. Well, obviously if all other conditions of the apparatus remained the same and there were only one-half oscillation in the primary instead of four or five half oscillations, the voltage which would have existed on the dummy antenna of the condensers would have been very much smaller. It could not have been anything like as great as that which it was.

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Q. From your experience as a designer and constructor and user of wireless telegraph transmitters, what can you say as to the presence of coupling waves or beats, what are they the sign of, what are they an indication of? A. The existence of beats or impure waves in a transmitter is invariably the indication of some improper adjustment or condition of the set, or some of its parts. It may be that the spark gap is not in good condition. It may be that the adjustment of coupling is too close for the particular spark gap which forms a part of the set. It is the normal practice to adjust any wireless transmitter until a wave of satisfactory purity is 10022
obtained. That means until the beats are substantially eliminated.

Q. You have testified that you made tests on the Simpson mercury valve transmitter before it was sent to the Cruft High Tension Laboratory? A. Yes.

Q. In what condition was the particular Simpson set when you sent it to the laboratory of Dr. Chaffee? A. It was in exactly the same condition as it was when I received it, as far as I am able to tell. It was in good working order.

Q. Had you subjected it to any abnormal or abusive tests? A. No, I had been especially careful not to do that. The tests which I had made on it were of the very 10023
briefest possible duration, to enable me to get certain information that I wanted, and the actual amount of use which the set had before I sent it to Dr. Chaffee was very small. It would be less than the set would get normally in a couple of days' use on shipboard.

Q. Before the set was sent by you to Dr. Chaffee did you open the spark gaps? A. No, I did not.

Q. Did you when you were at Dr. Chaffee's laboratory at the Cruft High Tension Laboratory of Harvard University see the spark gaps opened, the spark gaps of

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the Simpson mercury valve transmitter? A. Yes, I saw them opened twice.

Q. What condition were they in when they were first opened? A. They were in bad condition when they were first opened, two of them, at least, had quite bad pits, burned places in them. One of them was less bad, but still not in good condition. The other two were substantially all right.

Q. Is it an unusual thing for the spark gap to become pitted by use? A. No, unfortunately it is not unusual. It is very common even with the best types of spark gap
10025 and the best types of transmitter. The pitting of the gaps is a thing which happens and which as yet we cannot get rid of. In this particular set, due to the relatively enormous capacity and the fact that one of the electrodes of the gap has very small cooling surface, the tendency to pit is tremendously greater than it is with most quenched spark gaps and most transmitting sets.

Q. So that pitting and burning of spark gaps in transmitting sets is something that ordinarily would be expected in ordinary use? A. Yes, it is a thing which we encounter regularly. It has to be taken care of.

Q. What was done after the spark gaps were opened at Dr. Chaffee's laboratory? A. The spark gaps had
10026 their sparking surfaces, the silver surface turned off smooth in a lathe until their surfaces were perfectly smooth and parallel, and until the distance between the two sparking surfaces of the gap was exactly the same as they were originally. This distance was about .006 of an inch, and the gaps before they were turned off were measured to find out just what this distance was.

Q. And from your experience as a designer and constructor of wireless telegraph transmitters was this treatment a proper treatment so that the apparatus could be fairly tested? A. Yes, certainly. It was the thing that every one has to do with a quenched spark gap when it pits up. It is the regular procedure under these circumstances.

Q. Now, you said you saw the spark gaps opened the second time, when was that? A. Well, that was at the completion of the tests, the second day of the tests. I forget just what the date was.

Q. What was the condition of the spark gaps when they were opened the second time? A. They were in very good condition. They had not been damaged in any way at all. They were perfectly smooth.

Q. Did you observe whether or not the two halves 10028
of each gap were colored? A. Yes, I did.

Q. Were they colored symmetrically or unsymmetrically? A. They were about the same. I did not note any differences between the two different plates.

Q. What does the color on spark gaps or both sides of spark gaps indicate? A. Well, it indicates that a spark has been taking place. It shows the extent of the surface on which the sparking has been taking place, and if the sparking is uniform. If the little marks and mottling is uniform all over the sparking disk it means that the spark has been traveling around regularly and spread over the surface as it should.

Q. When you examined the sparks of the Simpson 10029
mercury valve transmitter on the second day and for the second time, what evidence was there of any arcing or burning? A. There was no evidence at all on the second day. The plates were in perfect condition.

Q. As one skilled in the testing of wireless telegraph apparatus what have you to say as to the reliability of the method of test described by Mr. Simpson at the University of Washington, where the connections of the Braun tube were across the spark gap? A. Well, that particular method was an absurdity on the face of it.

10030 The tube was connected across two points, which, prior to the spark, would have a voltage of three or four thousand volts between them. Now, that would be represented in the deflection or in the distance that the spot of light was thrown out, by—say an inch. Now, up to that time, the time when the spark starts, there would be no oscillations. They happen after the spark starts. But the instant the spark does start this space across the terminals of the gap which had previously been of comparatively infinite resistance now becomes a good conductor. The space is a conductor, and due to the
10031 very large current in this case, a very good conductor, so that the potential across the spark gap which was originally 3,000 or 4,000 volts now becomes probably something less than 100 volts. While I do not know exactly, my guess would be that it would be something of the order of ten or fifteen volts. Well, if an inch of deflection represented a voltage of three or four thousand then it is easy to see that eight or ten or twenty or even one hundred volts are not going to be represented on that scale at all. It is a good deal like in drawing a map. If you attempt to show the size of a building, we will say this court house building, on a map of ordinary size which has the entire world shown thereon, it is easy to
10032 see that on that scale the space occupied by this building would be of such dimensions that you would have to find it with a microscope, and exactly the same thing existed in this test at the University of Washington with the Braun tube. There might have been ten million oscillations there, but that test would not and could not have shown it because the distance that they would have deflected the beam would have been microscopic in comparison to the distance which the charging voltage deflected it. So that, referring to this particular photograph marked "Simpson photo by Braun tube" whatever

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oscillations actually did take place are all concealed in the blurred spot which indicates the center or zero. They may be there, but we cannot see them. And that is the reason why that particular method of test had no chance at the start of showing what the facts were. That connection to the outside plates was also an improper thing to do. Dr. Chaffee has brought out in his testimony that fact, but personally I do not know enough about the Braun tube to say of my own knowledge anything with respect to that part of it.

Q. You just referred to the photograph which is marked "F.G.S.—4" (Vol. 2, p. 435) and to Figure 1 of 10034
that photograph, did you not? A. Yes, Figure 1, or any figures, for that matter.

Q. From your experience in testing wireless telegraph apparatus what does Mr. Simpson's method of test as conducted by him at the University of Washington prove, if anything? A. Well, it simply proves that the condenser was charged. The spot of light has moved in all of the photographs, and it has moved under the influence of that charging voltage, so that it shows trat the condenser has received a charge. But it does not show anything of what happens after that.

Mr. Betts: That is all. You may cross examine. 10035

CROSS EXAMINATION.

Q. (Mr. Farnsworth.) Do you know how many oscillations there are on the antenna circuit of the Simpson mercury valve transmitter? A. Well, that would depend entirely, Mr. Farnsworth, upon the antenna you connect it to. There may be one or there may be a thousand.

Q. Have you ever seen a Simpson mercury valve transmitter in commercial service? A. No, I have never seen one actually operating in commercial service. I have

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seen one in the process of installation for that purpose.

Q. Take an ordinary ship installation with the Simpson mercury valve transmitter; can you estimate approximately the number of oscillations in the antenna circuit? A. Oh, my guess would be 40 or 50. That is down to one per cent. of the maximum.

Q. That is the ordinary way? A. Yes, that is one of the ordinary ways of referring to it.

Q. The Massachusetts witnesses of the plaintiff figured on Dr. Chaffee's dummy antenna with the Simpson mercury valve transmitter that there were over 50,
10037 between 50 and 60 oscillations in the antenna. Will you show those on your chart R. A. W.—4 on the same scale, your estimate of 50? A. I cannot show the same scale, because there is not room enough on the paper. I can show you that on a different scale.

The Court: You might mark those figures 1 and 2.

A. Yes, mark that Figure 1 that is on there now.

Q. I see that on that R. A. W.—4, in drawing the antenna oscillation to a different scale you have not shown the spark gap circuit, that is, on the same scale. Will you do that underneath on the lower part of the
10038-10044 Fig. 2. (Witness draws diagram.) (page 3336).

Q. Are you quite sure in your recollection that on July 4th last, in Massachusetts, the spark gaps were opened at the end of the tests on that day, after the completion of the July 4th tests? A. Well, I am not positive of the date, but at the close of the second day, after all of the tests were over, the spark gaps were opened, yes.

Q. You mean the only time the spark gaps were opened on the second day when the defendant's representatives were there was at the completion of all the tests on that second day? A. I have not said that the only time was then. I have said that they were opened after all the tests were made for the purpose of finding out what had happened to them, if anything.

Q. Not after all the tests were made and in the presence of the defendant's representatives? A. Yes, most certainly, certainly they were.

Q. Were they opened in the presence of the defendant's representatives on the second day, namely, July 4th, at any other time than after the completion of all the tests? A. Well, that I do not remember. It may 10046
have been. I did not pay any particular attention. They were opened at the conclusion of the tests to show the condition of the gaps during the tests, and after we had gotten all through with the tests.

Q. That is your recollection, and on that occasion when they were opened after the completion of all the tests, they were then opened in the presence of the defendant's representatives? A. Yes, or at least shown to them. I do not at this minute recollect whether defendant's representatives were present at the actual opening. I know they were shown to them, because I was present and recollect that, but I do not remember exactly whether or not representatives of the defendant were present at 10047
the actual opening.

Q. In the opening of those spark gaps after the first day's tests, on July 3, prior to the second day's tests on July 4, what happened to the spark gaps in the act of opening them; I refer to the insulation or so-called gaskets inside the gaps? A. In opening two of the gaps which were very badly pitted the gaskets were damaged. That is all that I think of.

Q. Yes, and in an endeavor to compensate for that breakage what was done? A. When the gaps were put together after they had been properly and normally turned and trued up, after they were put together the space which the gaskets, or some of the space which the gaskets had filled before it was broken was filled with sealing wax, the idea being to get as near an air-tight joint as possible.

Q. Now, Mr. Weagant, after the gaps were opened after the completion of the first's days tests on July 3, and before the tests on the second day, on July 4, I understood you to state that the spark gap surfaces were turned down with a lathe, is that right? A. Yes, that is correct.

10049 Q. So that when those gaps were operated on the second day's tests they were operated with those fresh, turned down surfaces of the gaps? A. Yes, they were.

Q. Now, referring to the pitting of the gaps, do you refer by the word "pitting" to those enormous craters and bulges which were found on opening those gaps after the first day's tests, on July 3? A. Those are a form of pitting, yes, a very common form.

Q. Do those craters and corresponding bulges now appear on the gaps? A. No.

Q. Why not? A. They were turned off.

Q. That generator, motor generator, which was used in the Massachusetts tests of the Simpson mercury valve
10050 transmitter, that is one of the type regularly employed by the Marconi company in its radio transmitters? A. No, it is not.

Q. What did the Marconi name plate on it mean? A. Simply that the Marconi Company owned it.

Q. It never used any others like it in its radio tranmitters? A. Oh, yes. Yes, I think we have used some of the machines like it.

Q. Made for the Marconi Company by the Crocker-Wheeler Company? A. Well, yes, of course, we bought them from the Crocker-Wheeler Company.

Q. Do you know when or where or under what conditions those spark gaps used in the Massachusetts tests became so pitted, as you call it, or at any rate assumed the condition which made it necessary for you to repair or reconstruct; when and under what conditions did that happen, do you know? A. No, I do not, except that the necessity for opening them was made evident on the first day of the tests. The set operated irregularly and it was found that two of the spark gaps were short-circuited. Now, they had not been in that condition, I think, the day before, so that evidently it had happened about that time.

Q. That is, it happened on the first day's tests, July 10052
3? A. Apparently.

Q. About those resonance tests which you say you saw, Mr. Chaffee make; can you produce any resonance curves or record of those tests that you saw? A. I haven't said anything about resonance curves. I haven't said a word.

Q. I think you said you saw Dr. Chaffee make some tests as to the resonance conditions of the Simpson mercury valve transmitter. A. I said I saw him make tests as to the condition of resonance between the two circuits.

Q. I will ask you if you can produce any curves showing the results of such tests? A. No, I cannot. We did not plot any curves. It was not necessary. 10053

Q. We have no record here today of what occurred at those tests? A. Nothing other than my statement, no.

Q. You are certain that the spark gaps were not opened on the second day of the tests, on July 4, after some tests on July 4, and before the completion of all of the tests? A. I have not said anything of the kind, Mr. Farnsworth. I said I knew they were opened at the completion of the tests and prior to the starting of the tests. Now, whether or not they were opened during the tests, some intermediate time, I do not recollect. I do not think they were, but I am not positive on that point.

Q. You are familiar with types of radio telegraph transmitters where the antenna is directly charged, are you not, such as the Poulsen system, or the systems where the antenna is directly charged by high frequency alternating current. If so familiar, Mr. Weagant, isn't it a fact that in such cases of the direct charging of the antenna, as much as 50,000 or even 100,000 volts may pile up at the upper end of the antenna. A. Well, the question is not at all clear as it stands.

10055 Q. That is, I mean, of course, when the charging voltage is down as low as 600 or 1,000 volts? A. Well, I am not particularly familiar with either the Poulsen system or that method of operation which consists of connecting a generator directly into the antenna. I am somewhat familiar with them, however, and I know their principles of operation, and I do not think there is any parallelism whatever between either of those cases and the Simpson mercury valve transmitter.

Q. Yes, but what I am asking you is, with the charging current as low voltage as 600 or 1,000 volts, nevertheless, the antenna is charged and directly charged as high as 50,000 to 100,000 volts at the top of the antenna.
10056 A. Yes, but the antenna in the first place is not charged at the rate of 240 times a second, or 1,000 times a second, as it would be in the case of the Simpson transmitter, but at the rate of maybe 100,000 times or 75,000 times. Then again, you leave out of your question the all essential thing in that particular proposition, namely, the method, or rather, namely, just what the voltage that you apply to such a system means. Now, I will explain that in this way; supposing that in an antenna we connect an alternating current generator of very high frequency. Now, it is true that that alternator may at its terminals generate

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only two, or three, or five hundred volts potential, whereas, at the terminals of the antenna there may theoretically be hundreds of thousands of volts, but that whole circuit is tuned to resonance with the frequency of the alternator, and what the alternator is supplying in the way of voltage is simply that voltage which is necessary to overcome the resistance of the antenna. Now, in the case of the Simpson mercury valve transmitter you are putting your voltage in across a condenser. Now, that is the essence of the difference. In the Simpson mercury valve transmitter arrangement you have got to get the
antenna potential way up high, yet if it were charged 10058
directly, as your question implies, it would have the potential of the large condenser, which we have called c, some three or four thousand volts, and that is all the voltage you would be putting across the antenna. Now, that particular proposition and the two illustrations which you have just cited differ fundamentally and absolutely in that respect. That is the ordinary, everyday, well-known fact of a circuit resonant to a given frequency.

Q. Would you say, therefore, in the Poulsen transmitter the antenna is in resonance with the direct current generator supplying the arc in the antenna? A. Well, I do not think the question as stated means any-
thing, Your Honor. 10059

(Question repeated to the court.)

The Court: I think we are going a little far afield in that.

Mr. Farsworth: That is all I have on that line, Your Honor.

Q. Before the days of practical use of impulse transmitters, Mr. Weagant—and you were familiar with the old, now abandoned systems of persistent oscillating primary circuits, I take it, were you not?

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Mr. Farnsworth: I want to ask him if the number of oscillations in the primary circuit of those old types of transmitters were not substantially the same as the number of oscillations in the antenna.

A. Referring now to what sort of transmitter, Mr. Farnsworth?

Q. The transmitters commercially used before the impulse type of transmitter just now in use. A. Well, I will answer it this way; first of all I do not recognize that the term "impulse transmitter" is one commonly employed
10061 in the art to distinguish old from modern apparatus. There never was a time when, as it was looked at by the people working in the art, the impulse transmitter did not exist, and a time subsequent when it did. That whole thing is a matter of slow development, slow change from one thing to another, and insofar as the earliest apparatus with which I am familiar is concerned, my recollection of it is that when properly used the number of oscillations in the antenna circuit was very much greater than the number of oscillations in the closed or spark gap circuit. I have never, as far as I can recollect, measured a transmitter in which there were more oscillations in the pri-
10062 mary than there were in the secondary, and in fact in which there were anywhere near as many in the primary as in the secondary. That condition of comparatively few oscillations in the primary and a large number in the secondary has been characteristic of every single wireless transmitter that I can recall at this time that I have ever seen.

Q. Before the law compelled the Marconi Company to use a transmitter without two wave-lengths in the antenna. Did you ever measure one of those old transmitters? A. Yes, I have, Mr. Farnsworth. I have measured quite a few of them as I recollect it, and, as a matter of fact, the number of oscillations in the primary of that particular sort of transmitter, as indicated by the methods which we had at the time, was very few in comparison to the number of oscillations in the antenna circuit.

Q. You were then in the employ of the Marconi company? A. Why, yes. I do not recollect of having made any measurements of their apparatus when I was not in their employ.

Q. When did you go in their employ? A. 1912, in the spring. 10064

Mr. Farnsworth: That is all.

The Court: Any redirect examination?

Mr. Betts: No, sir.

(Witness Excused.)

Mr. Betts: The plaintiff now rests its case in reply to the defendant's evidence heretofore entered of record.

Defendant's Motion for Surrebuttal Tests and Rulings Thereon. 10065

Mr. Skeel: If the court please, I wish to first renew my motion for the check-up tests on the receiver that I have referred to twice. The court will recall that on last—I think it was last week—I appeared before Your Honor and made a motion that these tests be held on Monday so as to save the time of the court, but it then appeared that Mr. Betts and his engineers would not be present, or be in town at the time, or if they were here they

10066 Defendant's Motion for Surrebuttal Tests and Rulings Thereon.

would be engaged in other work, so the matter was passed. I make the motion at this time so that we can select the most convenient time for these tests, and I will say they will not take to exceed a day, and I think less.

Mr. Betts: I should like to be heard upon that motion, if the court please, and I should like to have counsel state upon what ground he now makes the motion.

Mr. Skeel: Yes, I will do so, if the court please,
10067 and I must say that I am surprised that our request for a test is not acquiesced in by the attorneys for plaintiff in view of the fact that these tests were expressly promised in the presence of the Assessors by Mr. Weagant, the engineer of the plaintiff company at the time of the taking of the other tests.

Mr. Betts: No, Mr. Skeel.

Mr. Skeel: That will be duly shown in the record. I will now quote from Mr. Marriott's memorandum at the time the tests were made:

"May 17, 1916, at about test No. 52, Mr. Thompson requests former complete series with gal-
10068 vanometer."

These are Mr. Marriott's notes:

"At test 59, one of Thompson's tests came in here and Mr. Thompson requested it, but plaintiff said they would not do it now, but it could be gone back to later. The test corresponded to former test No. 35."

"At test No. 64, noted by R. H. Marriott; Tests No. 43 and 44 came in here (postponed)."

"Note made by R. H. Marriott as tests were concluded:"

I quote now from Mr. Marriott's notes.

Mr. Betts: Are these notes part of the record in this case?

Mr. Skeel: No. They will be part of the record when we call Mr. Marriott as a witness in this case. And I want to call the court's attention to the fact that these other tests were made without our expert being in attendance. As you recall, during the presentation of defendant's case the plaintiff was taking their tests, and Mr. Pickard had to be on the stand. We had no attorney present, and we
had only Mr. Thompson present, and, therefore, 10070
when the report of the tests was made we were not as familiar with what transpired then as we are now. This is the note which I asked Mr. Marriott whether or not it was made at the time, and he stated that it was. That is, I asked him what notes were made of Mr. Thompson's requests, and he gave me this memorandum:

> "Mr. Thompson made general request for tests, but Weagant said he did not want to stop tests; he could have opportunity after they were through."

So, if the court please, the procedure was adopted of 10071
allowing the plaintiff to finish those tests on the distinct understanding that upon the conclusion the defendant would be allowed reasonable check-up tests.

Now, at the conclusion of the Assessors' tests the plaintiff took the apparatus over to the Maritime Building and there conducted tests upon the Thompson transmitter, and then came to court and the Assessors were examined, and immediately after that the other evidence went in, and the case was postponed and adjourned without any opportunity having been afforded for these check-up tests.

10072 Defendant's Motion for Surrebuttal Tests and Rulings Thereon.

Now, briefly, if the court desires to have stated the purpose of these tests—I am not sure that I can state them technically, particularly in view of the fact that I was not there, but the general purpose is this; the plaintiff took one of defendant's receivers and used three condensers, substituting condensers furnished by the plaintiff. Of course, the defendant's receiver had no condenser at all. The plaintiff furnished three separate condensers and inserted those condensers on the receiver so as to make the secondary circuit a substantially oscillating circuit, and they tested the receiver comparatively as to wave-
10073 lengths without the condensers and with the condensers for the purpose of ascertaining the comparative intensity of signals, and showed results purporting to show, from which conclusions were drawn that on wave-lengths between 300 and 600 meters there was no difference in whether the condensers were on or not, that there was in substance no difference, I believe, between 600 and 1200 meters, and they thereupon purported to show that after 1200 meters was passed, and further, up to 3600 meters, there was a falling off in efficiency. The fact is, I am advised, that this matter could be predicated upon one of two grounds, either upon the use of resonance or upon
10074 closer coupling, and I am informed that in the tests conducted with the condensers the circuits were adjusted to resonance for the purpose of getting the most intense signal that could be secured, and that when the condensers were not used that then the most advantageous coupling that could be secured was not secured. In other words, we are prepared to show that the question of coupling has not been gone into in this case with respect to its bearing upon the intensity of the signals. Also, if the court please, the following statement was made by Mr. Weagant on page 2186 in his testimony, when asked the following question:

Defendant's Motion for Surrebuttal Tests and Rulings Thereon. 10075

"Q. If the defendant's receiver had its secondary so constructed as to have a natural period of say 200 meters or less, how would defendant's receiver operate, in your judgment, to receive wave-lengths of 600 meters? "A. It would operate very poorly, so poorly that I do not think anyone would ever think of using it for commercial purposes. I might say on this point that I have built coils having nearly all of the natural periods that have just been referred to, and I have used them on all wave-lengths within the present used 10076
range, and the statements I have just made with respect to them are based upon actual observation and long experience with these effects."

Now, the pertinency of that testimony is this, if the court please, that the evidence was introduced to show that the only fixed time period of the defendant's receiver was approximately 370 meters. Of course, the testimony is that there is no wave length of that length used for actual signaling purposes. The plaintiff endeavored to show that by reason of the fact that it was that length there was a broad tuning, as Mr. Weagant expressed it, between 300 and 600 meters, and then he goes on to testify 10077
that if this had been put out, that range of broad tuning and tuning as low as 200 meters, that the receiver would have been absolutely inefficient and of no use for practical purposes. We have constructed a coil of 200 meters, which we simply insert in the receiver so as to make that circuit the secondary time period of 200 meters instead of 370 which is the approximate ordinary time period of the circuit, and we will demonstrate by using that that Mr. Weagant's statement is wrong, and if it is wrong the whole testimony in regard to broad tuning is of absolutely no force or effect.

I am now giving Mr. Hughes the notes of Mr. Marriott's statement on that point. Perhaps I should have had that in, but I did not know whether the court remembered that those tests of the plaintiff were made while our own case was going in, and we had inadequate representation there.

Mr. Betts: Now, if the court please, as I understand it from Your Honor's ruling, which is in absolute agreement with the rulings of all the Federal courts I have ever practiced before, it is
10079 this; that surrebuttal testimony will not be allowed to either party as a matter of right, but rather as a matter of grace, and that further the surrebuttal testimony will not be allowed either party unless some new matter has been put in by his adversary. That was Your Honor's observation, I think, the day before yesterday, and it is quite in accordance with the rulings of the Federal courts.

What does the record show, if the court please, in regard to tests on defendant's receiver? Well, it shows, in the first place, that Mr. Kolster conducted tests on the defendant's receiver on the
10080 6th of April, and that Mr. Kolster testified in regard to that beginning at page 1376, (Print, p. 1492, Vol. 3) and Mr. Pickard at page 1484 (Print, p. 1587, Vol. 3). Then our adversaries wanted to make a comparative test between their receiver and our receiver, and we acceded to that and asked the court to appoint Assessors, and Your Honor, I have no doubt, remembers very well the fact that we left this room and went down to the radio room in this building where tests were made by Mr. Thompson and Mr. Weagant on both receivers in the presence of the Assessors and of this court.

Defendant's Motion for Surrebuttal Tests and Rulings Thereon. 10081

Both Mr. Thompson and Mr. Pickard have already testified again fully with respect to that, pages 1793 and 1821, (Print, pp. 1904, 1927, Vol. 3) respectively. So that was two sets of tests which the defendant initiated. Then we asked for some tests to be taken by us in the presence of the Assessors in reply, rebuttal, and those tests were conducted by the Assessors in the presence of Mr. Thompson or Mr. Pickard or both, and the Assessors made their reports. That, I believe was about the 15th or 16th of July. Now, at the con- 10082
clusion of this trial, if the court please, on the last day of April, Your Honor very clearly indicated that the proofs on both sides as regarded the receiver and the Thompson transmitter were to be considered closed. That is, you said that you wanted that aspect of the case finished, so that when we resumed here in July we would take up the Simpson mercury valve transmitter, and Mr. Skeel was asked by Your Honor whether he desired to take any surrebuttal testimony on the Thompson transmitter and the receiver, and what he asked leave to do was to take surrebuttal testimony to show with respect to the Thompson trans- 10083
mitter that the customers of the defendant, neither the customers nor the defendant itself had ever substituted any other loops than the ones put out on the market, and, second, with regard to the receiver that neither the defendant nor any of its customers or users had actually connected the condenser across the secondary. There was not a word or a suggestion made to this court at that time when this case was adjourned in regard to any further tests on the receiver, or further tests on

the Thompson transmitter, and I am confident the court remembers clearly that the case was to be considered closed with those two exceptions, on those two points. The court said on page 2888 of the record in answer to Mr. Hughes, when he said, "No, I want to close this apparatus now and if any further testimony is to be presented a showing must be made of almost sufficient to grant a new trial." Hence I say there has been no showing that any matter has been brought in by the plain-
10085 tiff in rebuttal, consequently, that the defendants have already conducted and participated in three series of tests, and, third, that there was no suggestion made at the close of this trial on the 29th of April of any further tests, and the court, I am sure, remembers distinctly the testimony as to the receiver and the Thompson transmitter was to be closed. And Your Honor will note that in our testimony which we have adduced this week it has been solely limited to the Simpson mercury valve transmitter in conformity with Your Honor's ruling. I, therefore, think that this re-opening of the standard receiver of the defendant will only pro-
10086 long the trial, because if they conduct tests with some specially constructed coil which Mr. Skeel has just referred to it may be that we shall have to ask for further tests upon that. I think this trial has been prolonged enough. The testimony is full and clear on the standard receiver of the defendant.

Mr. Skeel: If the court please, I have not read the proceedings of the last day of the trial today, but, nevertheless, my memory upon the matters therein discussed is very clear. What happened

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was this; the plaintiff was in the midst of its rebuttal, and it finished its case as to the receiver and as to the Thompson transmitter, and then it made a motion for a continuance and we argued that motion. It was along about 2:30 or 3:00 o'clock Saturday afternoon and the question was raised as to whether we could use up the afternoon by putting in some evidence that we would have in surrebuttal on the Thompson transmitter and the receiver, and I thereupon stated that we
had some witnesses that could testify to the facts 10088
just suggested by Mr. Betts, that is, that no such loops or condensers were ever used, and I proposed to call witnesses for that purpose, and the court stated that the plaintiff having finished its case would not be allowed to produce any more evidence on those matters unless they showed grounds almost sufficient to grant a motion for a new trial. But there was not the slightest suggestion that the defendant would not be allowed to answer the new matter that had been brought up, and that is precisely what we propose to do. The plaintiff, over the defendant's objection, tested not our re-
ceiver, but a receiver with some additional con- 10089
densers, and a transmitter with some additional loops. We did not deem it necessary, if the court please, to answer the testimony as to the transmitter, because that testimony, the tests that were conducted there fully support our theory, and we think the tests upon the receiver, even with the additional condensers, fully support our theory, but that was new matter and we have a right to check up those tests in accordance with the agreement at that time.

10090 Defendant's Motion for Surrebuttal Tests and Rulings Thereon.

Mr. Betts: I do not understand there was any agreement about any check-up tests, and I never heard of any proposal to take any surrebuttal tests until I got the telegram—Mr. Farnsworth read me a telegram from you in Boston. I will ask anybody to refer in the record to where any suggestion was made of any surrebuttal tests, and I think Your Honor's recollection will bear me out on that point.

Mr. Hughes: I would like to suggest to the court before the court passes upon the matter; there is one consideration that ought to be in the mind of
10091 the court in ruling upon this matter. We have discovered that the effect of testing, and the results of testing heretofore done on both sides was that one test simply leads to another. That sort of thing ought to be limited, and it is hardly conceivable to my mind that they will introduce a series of tests which will not in like manner suggest a series of tests that our witnesses will again say are necessary to explain the significance of theirs and show that they were improper or meaningless, as the case may be. What I want to call the court's attention to is that if this field is opened and further
10092 tests are had before the Assessors, it is almost inevitable that it will lead to a like request from us to make other tests suggested by the tests they make, and which our engineers will deem essential to explain those tests.

The Court: I recall the proceedings in court substantially in accordance with the statement of Mr. Betts with relation to the closing of the testimony upon the issue created by the Thimpson apparatus. I understood, however, in a certain way that there was some rebuttal or surrebuttal to

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be presented on the part of the defendant. Now, I do not recall just what it was, but I do not recall that any of that rebuttal was to comprise any further tests. I feel, however, this way with relation to the request made; we have consumed, as we all know, a very great deal of time, and if there is anything else that should be presented with relation to the Thompson transmitter that will elucidate any fact which is in issue here or will develop, a fact which the court should consider, I believe
that it is the duty of the courts to receive it. If what 10094
is stated as having transpired at the time the test was made, and it was understood, or whether understood or not, that some further tests would develop or throw more light upon the tests which were actually made, then I think that the suggestion being made now before the case is closed that the court in all fairness should receive it. If this further test should develop some other tests I am not saying that further tests would be foreclosed, but it is made apparent to me that possibly these further—that is, from what was said in court here —that possibly the further test would elucidate
some fact, and if so, we ought to receive it, and if 10095
this test would make necessary some further test then perhaps that should be made. But I would suggest that these be made as speedily as possible and without any delay. Now, as I stated yesterday, we will not have any session of court in this proceeding Saturday, and if that can be utilized for those tests it would be very proper to do so. I will state that in concluding upon this request for further tests I have taken into consideration the fact that Mr. Weagant and Mr. Waterman are both

here, so that the plaintiff is not placed at any disadvantage in the further tests which are made.

Mr. Hughes: There is one matter to which I think it is proper to call the court's attention, in view of what has transpired here. It seems from counsel that one of the Assessors has become disqualified to act as a disinterested party, because counsel has stated he expects to call him as a witness, and expects to prove certain things by him as their witness, and they have his confidential notes
10097 in their possession. If that is true he probably would not stand in the attitude of an Assessor from this time forward.

Mr. Skeel: Mr. Hughes misunderstood me when I referred to calling him as a witness. I had in mind only to prove what I did not know at the time the Assessors were cross examined, to the effect that this statement was a part of his notes. That is the only purpose of that.

Mr. Hughes: Unless he is your witness how should you have access to his private notes?

Mr. Skeel: These are his official notes.

Mr. Hughes: Were they filed?

10098 Mr. Skeel: No, I do not know that they were filed. The Assessors made notes at the time of the hearing. Mr. Thompson wanted to know of me why this matter was not brought out. I said I had not heard of it and then he asked the same question of Mr. Marriott, and Mr. Marriott said he was not asked about it, and they were right in his notes, there on Mr. Marriott's notes of the time the tests were made, and I would like to call Mr. Marriott as a witness, but I had no intention of doing so.

Now, in conducting this test we would like to have the Marconi tuner 106 receiver, or any receiver that they have, and the same condensers that were used in the other test.

Mr. Betts: I know nothing about this. It is all sprung upon me now.

Mr. Skeel: I notified you, Mr. Betts, that is, I telegraphed you and asked you if you could not have the tests before we resumed, isn't that the fact?

Mr. Betts: You telegraphed Mr. Farnsworth who read a telegram to me. I said I would not
stipulate in regard to any tests until I got to 10100
Seattle.

The Court: Well, you gentlemen can arrange that.

(Whereupon the court takes a recess until tomorrow, July 21, 1916, at 10:00 o'clock a m.)

Friday, July 21, 1916. Continuation of proceedings pursuant to adjournment. All parties present, as at former hearing.

10101

Reading of Defendant's Surrebuttal Depositions.

(Whereupon counsel begin the reading of the depositions of Simon and Stone to the Court. See Transcript, Vol. 5, pp. 3372, 3390.)

Afternoon Session, 2:00 o'clock.

Mr. Skeel: It is stipulated between the plaintiff and the defendant that all of the exhibits attached to the respective depositions may be considered as offered in evidence at this time on behalf of the plaintiff and the defendant, respectively.

The Court: Admitted.

Mr. Betts: And as marked in the testimony.

The Court: Yes.

NOTICE OF DEFENDANT'S SURREBUTTAL TESTS AND STATEMENTS OF COUNSEL.

10103 Mr. Skeel: Also, I wish to give notice that the tests on the receiver will begin at 9:30 tomorrow morning at the defendant's laboratory, and I wish to ask counsel if we may have delivery this afternoon of the three condensers used by plaintiff in the prior tests, and also of the tuner No. 106—I understand the condensers have been delivered, but the tuner has not.

Mr. Betts: I told somebody that some of the condensers we used were part of the records in the case.

Mr. Skeel: No, they were not. We have the condensers now, but we would like the tuner.

10104 Mr. Betts: If the court please, as I understand Your Honor's ruling yesterday in regard to surrebuttal tests of the standard receiver of the defendant, it was to the effect that they could surrebut any evidence that we took by way of tests on the defendant's receiver, particularly as it had been agreed upon, which I did not know at the time, by Mr. Weagant or some one. Now, the court must bear this point in mind, which is perfectly clear, that we made no tests before the Assessors on plaintiff's receiver.

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The Court: You mean plaintiff's receiver?

Mr. Betts: On plaintiff's receiver.

Mr. Hughes: Solely on the defendant's.

Mr. Betts: All of our tests before the Assessors which this defendant has now asked the court for leave to reply to were made solely by us before the Assessors on the defendant's receiver.

The Court: Let me ask; the idea was to make your tests on the defendant's receiver.

Mr. Skeel: Well, that comprises by far the larger part of the taking of check-up tests. 10106

The Court: I will state frankly before I hear from either of you that I had in mind just the defendant's receivers.

Mr. Betts: Exactly.

Mr. Skeel: The purpose of the use of the plaintiff's receiver is simply this: Mr. Weagant, on page 2186 of the record, was asked the following question:

> "Q. If the defendant's receiver had its secondary so constructed as to have a natural time period of say 200 meters or less, how would the defendant's receiver operate, in your judgment, to receive wave-lengths of 600 meters? A. It would 10107
> operate very poorly, so poorly that I do not think anyone would ever think of using it for commercial purposes."

Then he goes on to say that he has actually built coils of that kind.

The Court: Let me see that question and answer.

Mr. Skeel: Yes, right there is the question and answer.

(Showing to court.)

10108 Notice of Defendant's Surrebuttal Tests and Statements of Counsel.

Mr. Hughes: That refers to the defendant's receiver?

Mr. Betts: Yes.

The Court: What were you going to say?

Mr. Skeel: This, if the court please; we have built our own receiver, that is, we have built a coil so that the detector circuit will have a period of 200 meters with the express purpose of proving that instead of the receiver being commercially inoperative that in fact it is better or at least equally as good. Now, you have got to compare it with
10109 something. That part of the test is very short. Mr. Weagant says that it would be commercially inoperative. We are willing to test it or compare it with any receiver which is on the market.

Mr. Betts: Now, if the court please, what I was about to say was this; your honor will recall in the radio room tests there was a comparative test or series of tests conducted before Your Honor and the Assessors of the plaintiff's and defendant's receivers, respectively, and those tests were completed by both sides. Now, subsequent to that time, we conducted tests before the Assessors sole-
10110 ly and only on defendant's receiver, and it is those tests which were brought to the court's attention yesterday by the memoranda of one of the Assessors, Mr. Marriott, who said that they wanted to supplement our tests on the defendant's receiver. Now, then the question propounded to Mr. Weagant, as Your Honor at once recognizes, was with respect to the defendant's receiver. Therefore, I say that any tests by way of surrebuttal to ours are as I said once or twice before solely limited to the defendant's receiver, should be limited to the defendant's receiver.

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Mr. Skeel: Now, Mr. Weagant has said that our receiver, the defendant's receiver, would not be commercially operative if the coil were 200 meters. We have made a coil having a natural time period of 200 meters, and we stand ready and willing to prove by comparison with the plaintiff's receiver or with any other receiver on the market that instead of not being commercially operative it is the equal if not the superior of any receiver, even with the coil clear outside of any recognized wave-length, as Mr. Weagant stated. 10112
Of course, if the court please, by merely using such a coil and by taking the ordinary measurements we can show that the receiver with such a coil would be commercially operative, that is admitted, but I wish to show how far the testimony of Mr. Weagant on that point was from the actual fact in the case. I may say that that part of the tests, the part where we use the receiver as a comparison, will be the very smallest part of the tests. It is a very small part.

Mr. Betts: If they want a comparison made, let them compare it with their own.

The Court: I think, Mr. Skeel, that the other 10113
matter was the only matter in my mind, and I am very reluctant to re-open the matter for further experimentation with relation to the plaintiff's receiver, concerning which no demonstrations were made.

Mr. Skeel: Yes, no demonstrations were made by the plaintiff.

The Court: So I think that the further experiments should be limited to the defendant's receiver.

Mr. Skeel: Then we will, however, be permitted to answer that statement of Mr. Weagant in our testimony, because that was a new statement not therefore in the record.

Mr. Hughes: I would like to call the court's attention in that connection——

The Court: We will cross that bridge when we come to it.

DEFENDANT'S SURREBUTTAL DEPOSITIONS READ IN EVIDENCE.

10115 New York, Monday, July 10th, 1916.

Depositions to be read at the trial of this cause taken on behalf of Defendant under order of this Court and pursuant to the United States Statutes, before George E. Brown, Notary Public, at the office of Philip Farnsworth, Esq., 149 Broadway, New York City, beginning July 10th, 1916, at 10:00 A.M.

Met pursuant to notice.

Present: L. F. H. Betts, Esq., and

10116 John W. Peters, Esq.,
For Plaintiff.
Philip Farnsworth, Esq., and
George F. Scull, Esq.,
For Defendant.

Emil J. Simon, a witness called called on behalf of the Defendant, being duly sworn, deposes and says in answers to interrogatories as follows:

Direct examination by Mr. Farnsworth:

Q. 1. Please state your name, residence and occupation? A. New York City, radio engineer.

Q. 2. You witnessed Plaintiff's Massachusetts tests on July 3rd and 4th last, and were present at all the testimony of the plaintiff's witnesses taken immediately thereafter? A. Yes.

Q. 3. Please state your qualifications in respect of testifying concerning those tests? A. I am a practicing radio engineer and have been engaged actively in this field for the past nine years, the former three of which were in connection with studies at Columbia University where I pursued the electrical engineering course.

I was assistant engineer to Dr. Lee DeForest three years during which time I was engaged in the develop- 10118
ment of the quenched spark type of radio transmitter which was introduced in this country by Dr. Seidt of Germany. This was in 1909. Since that time I have devoted myself almost exclusively to the development, design, manufacture and adjusting of quenched spark type of radio transmitting sets varying in power from one quarter to ten kilowatts. I consider myself thoroughly familiar with the mode of operation of this form of transmitter, and am likewise familiar with the various types of transmitters working on this principle and used in this country.

I have been a Fellow of the Institute of Radio Engineers since 1914 and have delivered several papers per- 10119
taining to radio telegraphy before that Institution, several of which have been published in its proceedings.

For the past year I have been designing and constructing radio telegraph sets for the United States Navy, which sets have been purchased from me as contractor by the Navy Department. These sets have been also of the quenched spark type.

Q. 4. You are a competitor of both parties in this case; that is, the Marconi Company and the Kilbourne-Clark Manufacturing Company? A. I consider myself such.

10120

Q. 5. Describe the history of the spark gaps as you observed them during the Massachusetts tests of July 3rd and 4th and during the succeeding testimony of Plaintiff's witnesses? A. According to my recollection the spark gaps of the transmitting sets tested at the Cruft Laboratory on July 3rd and and 4th, were opened and examined at three different times.

I examined one of the spark gap units after they had
been opened the first time. This was on the morning of
July 4th. The surfaces of this spark gap had previously
been machined in order to remove incrustations or pro-
jections which existed on the surfaces. I was told that
10121 this particular spark gap had been found short-circuited
and the sparking surfaces had welded. On examination
I found one of the sparking surfaces to have a circular
depression at one part of the surface of about three-
eighths of an inch in diameter, possibly one thirty-second
of an inch in depth. The rest of the surface was clean,
indicating that it had just been machined. The remain-
ing spark gaps at this time had already been put to-
gether, and I did not see their interior.

The second time I witnessed the opening of the spark
gaps and examined their interior was during the tests
on July 4th. As I recall it the set had been used for
approximately one hour, or, at the most, one and half
10122 hours, during the tests on this day, and prior to con-
ducting the tests with the antenna removed, these gaps
were dissembled and examined. I had an opportunity
at this time to note the surfaces on all of the spark gap
units. I had no way of telling what the distance between
the sparking surfaces was. The surfaces appeared oxi-
dized and indicated to me that the oxygen inside the gap
had not been entirely used, or else that the gaps were not
air tight.

The third time these gaps were opened was on Friday morning of last week during the deposition of Dr. Chaffee. I examined each of the sparkling surfaces this time and noted that they were thoroughly oxidized, indicating clearly to me that the gaps were not air tight. I had no means of determining what the sparking distance between the spark gap surfaces was. I noted all three occasions that at least one, and I believe on the last two occasions, two of the spark gap units had their insulation damaged.

Q. 6. You mean the insulating gaskets? A. Yes. The insulating gaskets were damaged. In these units this would partly account for their lack of air tightness. 10124

Q. 7. What was done, if anything, by the plaintiff's representatives in connection with the breaking of the insulating gaskets? A. I saw the gaps puts together the second and third time they were opened, and I do not recall that anything was done to replace the defective gaskets.

Q. 8. Or to attempt to remedy the trouble caused by the breakage of the insulating gaskets?

Mr. Betts: I object to that as leading.

A. I do not recall that anything further was done than what I mentioned in my answer to the previous question. 10125

Q. 9. What in your opinion is the significance of what you have testified concerning the spark gaps in its bearing on any results obtained in the Massachusetts Chaffee tests of the Simpson mercury valve transmitter? A. As it is generally recognized that the sparking distance between the opposing surfaces of quenched spark gaps largely determines their ability to function properly, or quench at the right time, and inasmuch as my experience has been that quenched spark gaps function better when they are air tight than when they are not air tight, it is

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my belief that these spark gaps were not repaired in a manner that would permit them to operate at their best efficiency; in fact several of the spark gap units I should say were probably unsuitable for further use in the transmitting set without making satisfactory repairs to them.

Q. 10. Please describe in detail just what Messrs. Weagant and Chaffee did during the Massachusetts tests of the alleged Simpson mercury valve transmitter? A. The tests were conducted by Messrs. Chaffee and Weagant. Dr. Chaffee paid particular attention to the operation of the Braun tube apparatus. Mr. Weagant con-
10127 fined his attention to the adjustment of the generator potential transformer reactance, and the tuning of the dummy antenna circuits.

A number of adjustments were made at the transmitter panel, and so forth, during these tests, and these were in some cases made by Dr. Chaffee and in some cases by Dr. Chaffee with the assistance of Mr. Weagant, according to my recollection. In no case do I consider the adjustments made were sufficient to put the transmitting set in its best operating condition as the apparatus existed there. If desired, I can give several reasons why I believe this to be so.

Q. 11. Give your reasons? A. As I understand the
10128 operation of the Kilbourne & Clark transmitting apparatus, it is essential that a node of potential should exist at the point to which the antenna inductance connects to the trigger circuit. I understand that the set is designed so that this may be done by a variation in the amount of inductance used in the trigger circuit and a clip is provided for this purpose. I very carefully examined the position of this connector prior to the commencement of the tests, during the tests and after their completion, and to the best of my recollection I do not believe this connection was varied from start to finish,

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although a number of different adjustments were attempted and made in other parts of the transmitter.

It is my belief that to accurately determine this node of potential an experiment or two must be made. This could be done, for example, by the use of a neon tube moved up and down, or along the coil, the position of minimum potential or node of potential being indicated by the non-lighting of this tube.

Another way that comes to my mind is the use of a buzzer for exciting the antenna circuit and noting the non-existence of sound in the telephone when connected between points corresponding to nodes of potential (one 10130
of which naturally is the ground).

The importance of obtaining this node of potential is apparent to me because otherwise it would be difficult to obtain quenching of the spark gap at the end of the first half oscillation. A node of potential at the point mentioned would materially facilitate quenching at this time because there would be no difference of potential existing across the spark gap. The point at which a node of potential would be obtained corresponds with the point where the capacity reaction of the condenser is equal to the inductance reaction of the inductance common to the antenna and trigger circuits.

As the capacity used is a fixed quantity for each of 10131
the wave lengths or tunes this point of zero potential could only be obtained by varying the amount of inductance common to the two circuits.

As stated before I do not recall that any attempt was made to vary this inductance during the tests on July 3rd and 4th.

Mr. Betts: The answer is objected to as incompetent, and secondary evidence, since it is based on merely the "understanding" of the witness as to what he thinks the Kilbourne & Clark transmitter set is.

A. (Continued) The adjustments that I noticed Mr. Weagant made were chiefly variation of the generator potential, which affected the excitation of the transmitter, varying the number of partial discharges which occurred across the spark gap. I very carefully noticed this effect in the stroboscope which was connected to the transmitter, by listening in the telephone of two different wave meters located near the transmitting apparatus. The spark, as noticed by observations of the stroboscope and by listening in the telephones, indicated to me that there were a large number of partial discharges present dur-
10133 ing practically the entire period of test on both days; although on the second day I think the adjustments were so as to decrease this number to a slight extent. This may have been due to the fact that some of the spark gap units were short-circuited or fused during the tests on July 3rd.

The tone in the telephones was practically at all times poor, although at times when Mr. Weagant made adjustments of the generator potential the tone became what I would say fair for a five hundred cycle quenched spark set. There were, I believe, one or two times during the test on July 4th when I particularly noticed the spark to be fairly good, but I am certain that at these times Dr.
10134 Chaffee did not take any photographs of the image on the screen of the Braun tube.

The tone was improved largely by the adjustment of the primary reactance by Mr. Weagant on the second day.

The band of light in the stroboscope was fairly wide, which also indicates that each discharge consisted of a considerable number of partial discharges. My experience with stroboscopes used with quenched spark transmitting sets is that a very narrow band of light appears when a perfectly clear tone, indicating the absence of partial discharges, is heard in the telephones. No such

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condition existed at any time during the tests on July 3rd and 4th. I can say this quite positively, and I believe I listened into the telephones of one of the wave meters, and noticed the operation of the stroboscope, at practically every time a photograph of the image on the screen of the Braun tube was taken by Dr. Chaffee.

In connection with my statement which referred to Mr. Weagant's adjustment of the antenna circult, I wish to say the following: the inductance which Mr. Weagant varied was supposed to represent in this dummy circuit the inductance of the antenna itself. Of course in the latter case this is a distributed inductance, and naturally 10136
could not be varied. In commercial use, therefore, on an antenna, the adjustment of the antenna circuit could not be made in this way.

On the second day's test I also noticed considerable brushing on the Leyden jars, which represented in the dummy circuit the antenna capacity. This brushing was also indicative of partial discharges occurring in the spark gap circuit, because the tone emitted by the brushing was not what I would consider a clear tone. The brushing on the Leyden jars has the effect of altering their capacity, and adds resistance to the circuit. It would be difficult, therefore, to actually determine by calculation the damping of this dummy antenna circuit. 10137

The ammeter used for noting resonance or proper operating condition, which it was stated Mr. Weagant attempted in each case to make, I wish to state that this instrument was not calibrated in amperes, and it was therefore impossible to say how much current existed in this circuit. The instrument was a low reading ammeter and was shunted by a piece of wire across its terminals.

Q. 12. What motor generator was used in those Massachusetts tests of the Simpson Mercury Valve Transmitter?

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Mr. Betts: Objected to as immaterial.

A. I examined the motor generator, and noticed it had a nameplate indicating that it was made by or for the Marconi Wireless Telegraph Company. On more careful examination, I recognized it as a machine built by the Crocker-Wheeler Company. The generator was of the so-called revolving armature type, and was five hundred cycle. The voltage which the machine was supposed to give, I noticed was marked on the nameplate 110-220. As there was no volt meter in circuit during the
10139 tests I am unable to say what voltage was actually used. I do, however, remember from Dr. Chaffee's deposition that he stated this voltage to be 125.

Q. 13. Did anybody vary that during the tests? A. The voltage was almost continuously varied by Mr. Weagant, as different tests were made or photographs taken, sometimes at the suggestion of Dr. Chaffee and sometimes by Mr. Weagant himself as a result of listening in the telephone of one of the wave meters, or by noting the operation of the stroboscope.

Q. 14. Give the frequency and any other properties of that generator you observed? A. I stated the frequency of the generator to be five hundred cycles. I am not able
10140 to state definitely any other characteristics, but I am of the opinion that it has a rather high synchronous impedance, as this is characteristic of all machines of this particular construction made for the Marconi Company. I also remember that the nameplate said that the speed of the machine was normally twenty-five hundred revolutions per minute. I do not recall whether the generator was a one or two kilowatt generator.

Q. 15. What, if anything, did you observe about the speed of the generator during the July 3rd and 4th tests? A. There was a motor field regulator in circuit, and this was located next to the generator field regulator operated by Mr. Weagant. I do not recall definitely whether or not this motor field regulator was varied during the tests. I noticed, however, that the speed of the machine dropped off somewhat under load.

Q. 16. By "load" you mean what? A. That is, when the key was depressed; particularly on the second day, when I believe the transmitter was subjected to more power than on the first day. The lack of indicating instruments in all parts of the circuit made it absolutely impossible to say how much power was being used at any point in the circuit at any time. 10142

Q. 17. Describe the two sets of wave meters used, and the stroboscope, briefly, for the purpose of explaining how you could tell the conditions? A. As I recall it, there were two wave meters available. One was the so-called Pierce wave meter, using a telephone with a silver diaphragm and operating on the eddy current principle. This is not usually considered a satisfactory form of wave meter for noting the spark tone. I therefore confined my attentions chiefly to the other wave meter, I believe one made by the General Radio Company, which used a crystal detector and telephones for receiving the signals, and here I was able to very carefully note their quality at all times. 10143

I did this in connection with a stroboscope, this latter instrument consisting of a small neon tube connected to a point in the dummy antennae circuit having sufficient potential to cause the tube to light. This tube is revolved by electric motor at a speed sufficient to separate the main spark discharges into lines, so that by the eye you can tell the number of principal discharges existing in the spark gap circuit per second.

Q. 18. In respect of that five hundred cycle Marconi generator used in the Massachusetts tests, and its mode

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of use during the tests, what can you say as to whether or not the same had any bearing upon the results of the tests, as to their accuracy or conclusiveness in respect to the activity in the converting trigger circuit of the Simpson mercury valve transmitter? A. My experience with quenched spark transmitting apparatus has taught me that the characteristics of the generator are of supreme importance if the set is to operate free from partial discharges and with a clear tone. The chief characteristic of the machine, of consequence, is usually called its synchronous impedance. Different designs of transmitters use generators having different synchronous impe-
10145 dances. Some effort has now been made by our Navy Department to standardize the synchronous impedance of generators for different sized sets. The set I saw tested at the Cruft laboratory had a so-called closed core constant potential commercial type transformer. Such a transformer, experience shows, has not sufficient leakage to eliminate the partial discharges which cause poor tone in the receiver, unless external inductance is used, either in the motor generator or in the form of a primary reactance, or unless some other means, such as I understand the function of the mercury vapor tube to be, is used. The variation, therefore, of inductance
10146 in the power circuit, or any change in the constants of this circuit, is likely to materially affect the operation of the set and the proper functioning of the spark gap.

Q. 19. Summing up your last answer, then, in reply to the preceding question, what will your answer be? A. That the characteristics of the generator play an important part in the proper functioning of such a quenched spark transmitter.

Q. 20. In respect of the conditions of the spark gaps during the tests, as you observed them and testified, what can you say as to the bearing of the same on the question of accuracy or conclusiveness of results obtained in the Massachusetts tests? A. As stated in one of my previous answers, I was not at all satisfied that the repaired spark gaps produced the necessary degree of quenching, or operated satisfactorily, during the tests on July 4th.

Q. 21. Summing up as to all the conditions of the Massachusetts tests observed on July 3rd and 4th, state your opinion, as a radio telegraph practitioner, as to whether or not the results there obtained showed anything accurate or conclusive as to the operation of the Simpson mercury valve transmitter under commercial 10148
operating conditions?

Mr. Betts: Objected to as leading.

A. I would say that the operation of the transmitting set, as I saw it on July 3rd and 4th, was far from what I would consider satisfactory. I believe that its operation could have been materially improved by carefully adjusting the circuits, and still further improved by replacing some of the apparatus with apparatus designed for use with this transmitter and in good working order. I wish to state further that the dummy antenna circuit which was used, in my opinion did not satis- 10149
factorily replace the normal open antenna.

Q. 22. As a radio telegraph practitioner, state your understanding of what is a transmitter of the impulse or impact type?

Mr. Betts: Objected to on the ground that the testimony taken under order of the Court was to be limited to observations made by this witness and other witnesses, if any, called by the defendant in regard to the tests of the Simpson mercury valve transmitter on July 3rd and 4th, but the

10150 present question is clearly outside of this scope and is a pure expert question.

A. My understanding of an impulse type of transmitter is one in which the energy of the spark gap's circuit is rapidly transferred to the antenna circuit and resulting in the projection of free oscillations in the latter circuit without any of said oscillating energy being transferred back to the spark gap circuit. The period of time during which oscillations exist in the antenna circuit shall be large in comparison with the time during which
10151 energy is flowing in the spark gap circuit.

Q. 23. I will now ask you a question in view of your last answer which is limited to the apparatus tested in Massachusetts on July 3rd and 4th which plaintiff apparently intended to represent to be a Simpson mercury valve transmitting test, and so far as those tests may have shown anything, state what type of transmitter the set then and there tested was shown to be? A. The Braun tube photographs taken during the tests on July 3rd and 4th together with depositions were made by Drs. Chaffee, Morecroft and others indicates that approximately two and a half oscillations existed in the primary circuit, and upwards of fifty in the antenna
10152 circuit. I would say that the transmitter falls within my definition of an impulse type of transmitter.

Q. 24. Did you observe Dr. Chaffee on July 3rd and 4th making the photographs of the images on the Braun tube screen? A. I noticed Dr. Chaffee taking a large number of photographs of images in the Braun tube screen on July 3rd and 4th. I did not see him take all of the pictures.

Q. 25. Were you listening to the notes of the signals or observing the indications on the stroboscope at any instance when you observed Dr. Chaffee taking photo-

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graphs of the images on the Braun tube screen? A. As previously stated, I noted the operation of the stroboscope and listened in the telephone receiver of one of the wave meters practically each time that Dr. Chaffee took a photograph of the image on the screen of the Braun tube, although it was impossible for me personally to observe the image at the instant the photograph was taken as the wave meter was some distance away from the tube, and as a number of the witnesses were crowded around the tube, obscuring it from my view.

Q. 26. How did you know the instance when Dr. Chaffee made the photographs? A. It was usually noticed by 10154
the dimming of the light in the room, and sometimes that he squeezed the bulb of the camera, although of this I am not very certain.

Q. 27. At times when you saw Dr. Chaffee squeezing the camera bulb, did you observe the condition of the note from the transmitter? A. On several occasions I did, both noted the note in the telephone receiver and observed him squeezing the bulb of the camera.

Q. 28. What is the fact as to the times you saw Dr. Chaffee squeezing the camera bulb as to the character of the note which you were observing at such instances? A. As previously stated in answer to some of your questions, the tone was usually bad. The few times when 10155
the tone was fairly good I very carefully remember how photographs were taken.

Q. 29. That is, as you observed, when was it that Dr. Chaffee chose to take photographs?

Mr. Betts: Objected to as leading.

A. I cannot say what he had in mind at the time he took the photographs, but I distinctly recall, as previously stated, that there were a few times when the note was best, and at these times I distinctly remember that there were no photographs taken.

Q. 30. Do you remember whether Dr. Chaffee took any photographs as you saw him squeeze the camera bulb at times or instances when you were observing that the note or tone was bad? A. Yes. As I said before, at times I noted the tone, and at the same time saw Dr. Chaffee squeeze the bulb; the note was not good.

Cross examination by Mr. Betts:

XQ. 31. Mr. Simon, you are the defendant in a suit
10157 brought by the plaintiff Marconi Company upon the Letters Patent in suit? A. The Marconi Company did bring suit against me on the Letters Patent in this case, but the District Court of New York dismissed the Bill and the Circuit Court of Appeals has affirmed the decision of the District Court.

XQ. 32. I did not ask what disposition the Court had made, but since you have volunteered information, I will further ask you to state if it is not a fact that the Supreme Court has granted a writ of *certiorari* on that case? A. My attorney has so advised me.

XQ. 33. Who paid your expenses on your visit to the Cruft high tension Laboratory at Cambridge, Mass.,
10158 when you witnessed the tests on July 4th? A. Mr. Simpson of the Kilbourne & Clark Company, I believe.

XQ. 34. And the Kilbourne & Clark Company are paying you for your expenses and time for testifying here today? A. I expect them so to do, as my time at the present moment is very valuable in connection with contracts I have with the United States Government.

XQ. 35. During the examination of Messrs. Chaffee and Morecroft and others, at Cambridge, Mass., on July 5th and following days, you suggested, did you not, to

Mr. Farnsworth, certain cross questions to propound to these witnesses? A. I did.

XQ. 36. How old are you? A. Twenty-seven and a half.

XQ. 37. When first did you put wireless telegraph apparatus on the market of your own design and manufacture? A. And in my own name, do you mean?

XQ. 38. Yes? A. I do not believe I ever did put wireless telegraph apparatus of my own design on the market; I do not know definitely what you mean by "on the market." My apparatus of my design which I have sold in my own name has been limited to sales to the 10160
War and Navy Departments of the United States.

XQ. 39. And when first was that business undertaken by you? A. Approximately one year ago.

XQ. 40. I believe you stated on your direct examination that you were a competitor of the Marconi Company, and also of the Kilbourne & Clark Company, the defendant herein. Do you manufacture for sale, or for lease, wireless telegraph apparatus for commercial use? A. No, I do not. I consider myself a competitor of the Marconi Company and the Kilbourne & Clark Company, because in the contracts I have secured from the Navy Department in the past year I had to compete in some instances with both companies, and I recall that in this 10161
instance both companies underbid me.

XQ. 41. When you test an apparatus of your own design do you use a dummy antenna or a commercial antenna for submission to the Government?

> Mr. Farnsworth: Objected to as vague and indefinite and not specifying the particular kind of tests referred to.

A. It depends all on what the nature of the tests are; if they are simple acceptance tests of standard ap-

10162 paratus whose operation is well understood, dummy tests usually suffice, but if I am investigating new apparatus whose functioning is not clearly understood, I invariably use an open antenna.

XQ. 42. When first did you see a Kilbourne & Clark transmitter of the Simpson mercury valve type? A. Some months ago at the Bureau of Standards in Washington.

XQ. 43. When first did you test, or participate in any test, of the Kilbourne & Clark Simpson mercury valve transmitter? A. By participating do you mean actually present at tests?

10163 XQ. 44. Yes? A. I believe at the tests conducted at the Cruft Laboratory on July 3rd and 4th, although I had previously been informed as to the operation of this transmitter, having read depositions made in this case by both plaintiff's and defendant's experts.

XQ. 45. What kind of a motor generator was connected to the Kilbourne & Clark Simpson mercury transmitter which you saw at the Bureau of Standards in Washington? A. My recollection is that it was one supplied by the Kilbourne & Clark Company and was of 120 cycles. Further particulars I do not recall.

XQ. 46. Who manufactures the generators for radio
10164 transmitting sets that you sell to the Government? A. In the past several manufacturers have supplied them. Among them I may mention the Holtzer-Cabot Company of Boston, the Crocker-Wheeler Company and the Diehl Manufacturing Company. These machines are designed specifically to meet the particular requirements of the form of apparatus supplied.

XQ. 47. And are these motor generators 500 cycle generators? A. These machines have invariably been 500 cycles.

XQ. 48. When Mr. Weagant made the adjustments to which you have referred in your direct examination, he did so by throwing certain switches, did he not? A. No; I would hardly consider throwing switches making adjustments.

XQ. 49. I believe in your direct examination that you expressed the opinion that it was desirable that certain apparatus should replace the apparatus used at the Cambridge tests on July 3rd and 4th. Will you please state what apparatus you had in mind in making that statement? A. I believe that was in answer to a question as to whether or not I considered the apparatus to be in
good working condition, and I had in mind at the time the 10166
motor generator, the spark gaps, the whole dummy antenna, and the reactance in the primary of the transformer circuit. This, I wish to be understood, refers particularly to the apparatus used, and not the adjustments of the apparatus.

XQ. 50. You have also expressed the opinion that the note of the Simpson mercury valve transmitter when it was tested in your presence on July 3rd and 4th, was bad, or fairly good. In making that statement, with what did you make a comparison? A. With a tone produced by a Standard 500 cycle quenched spark set in good working adjustment, the tone of which I am thoroughly familiar
with. 10167

XQ. 51. In your judgment, on which day was the Simpson mercury valve transmitter working best, July 3rd or 4th? A. It worked better on the 4th than on the 3rd, although on neither of the days do I consider its operation satisfactory.

> Mr. Betts: The last sentence is objected to as volunteered and irresponsive and showing the bias of the witness.

XQ. 52. Will you point out on Defendant's Exhibit F, G, S, 2, marked "Simpson drawings, the Simpson mercury valve transmitter" (Vol. 2, p. 435) the clip to which you referred in your direct examination, by which a node of potential is to be obtained. I show you a copy of this exhibit.

Mr. Farnsworth: I suggest that the witness be shown a copy of the testimony relating to and explaining this exhibit. With this testimony, of course, the exhibit must be read as the exhibit is merely a diagram.

10169 A. The drawing does not indicate the connecting clip to which I have reference, or had reference in my statement.

(Sgnd) Emil J. Simon.

John Stone Stone, a witness called on behalf of the Defendant, being duly sworn, testified as follows:

Direct examination by Mr. Scull:

Q. 318. Please give your name, residence and occupation? A. John Stone Stone, residence 34 Gramercy Park,
10170 occupation consulting electrical engineer.

Q. 319. You are the same John Stone Stone who has testified heretofore in this suit? A. I am.

Q. 320. Were you present at the Cruft laboratory, Cambridge, Massachusetts, while certain tests were made with an alleged Simpson transmitter on July 3rd and 4th, and did you hear or have you read the testimony of the witnesses Chaffee, Morecroft, Coffin and Cross given in this suit? A. I was present at those tests, and either heard or read the testimony of the witnesses referred to.

Q. 321. What are your qualifications that would ena-

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ble you to express opinions on tests such as you witnessed, of apparatus used in radio telegraphy? A. I began my technical education by studies of mathematics, physics and chemistry at Columbia University, and later studies of mathematics, electricity and magnetism and thermodynamics at Johns Hopkins University. In 1890 I entered the research laboratory of the American Bell Telephone Company at Boston, Massachusetts, as experimentalist, and continued such work in that laboratory until some time early in the year 1899. This work comprised research and design of systems of electrical transmission of intelligence, both with low frequency and with 10172
high frequency currents, and for both telegraphic and telephonic purposes. As early as 1892 I began experiments with high frequency electrical oscillations, and the electrical resonance of high frequency and low frequency currents.

About 1895 or 1896 I was appointed special lecturer at the Massachusetts Institute of Technology, lecturing to the graduating classes in physics and electrical engineering, upon the subject of electrical oscillations and their application. I continued to deliver these lectures over a period of six or eight years, I cannot remember exactly.

In 1899 after leaving the laboratory of the American 10173
Bell Telephone Company, I became a consulting electrical engineer, and have continued as such to the present day. Shortly after leaving the laboratory of the American Bell Telephone Company I resumed work with electrical oscillations of high frequency, and their application to radio telegraphy and telephony, and have been actively engaged in either experimental, practical or theoretical consideration of the subject of electrical oscillations, in their application to radio telegraphy or telephony, since that date. I have acted as expert in relation to radio telegraph and

telephone litigation for several concerns engaged in that business, in patent suits.

In the course of my work since 1892, I have been granted over a hundred U. S. patents relating directly or indirectly to radio transmission. I have read a number of papers before scientific technical societies relating to radio telegraphy and telephony and electrical oscillations. I have published papers in the technical press relating to radio telegraphy and telephony.

For a period of eight years, approximately, I was chief engineer of the Stone Telegraph and Telephone Company, a corporation engaged in the commercial ex-
10175 ploitation and technical development of my radio telegraph and telephone patents.

In the course of my work, both at the laboratory of the American Bell Telephone Company and later as director of the laboratory of the Stone Telegraph and Telephone Company, I have had occasion to make very many quantitative and accurate tests of radio telegraph apparatus and electrical oscillations.

I am a Fellow of the American Academy of Arts and Sciences, a Fellow of the American Association for the Advancement of Science, a Fellow of the Radio Institute; I was twice president of the Society of Wireless Tele-
10176 graph Engineers, and past president of the Institute of Radio Engineers; I have for several years been either president or a member of the standardization committee of the Institute of Radio Engineers; I am chairman of the wave length regulation committee of the Institute of Radio Engineers; I was a member of the International Electrical Congress that met at St. Louis in 1904; I was a delegate to the second Pan-American Scientific Congress held in Washington in 1915-16; I am a member of the American Electro-chemical Society and a member or associate of the following societies: American Insti-

tute of Electrical Engineers, U. S. Naval Institute, the Franklin Institute, the Society of Arts, and perhaps a few others I do not remember.

Q. 322. Please state where the papers referred to in your last answer as written by you were published, and their titles, so far as you recall them? A. I do not recall all of my papers, but the principal papers are as follows: A paper entitled "The Theory of Wireless Telegraphy" read before the International Electrical Congress, 1904, which was published in the Transactions of that Congress, and also in one or more electrical journals, including the Electrical Review, New York. 10178

The next paper of importance that I remember was one read before the Electrical Section of the Canadian Society of Civil Engineers at Montreal, 1905, entitled "Interference in Wireless Telegraphy." That paper was printed in the Transactions or Proceedings of the Society, reprinted in one or more of the electrical journals, including the Electrical Review, of New York.

Another paper was the presidential address before the Society of Wireless Telegraph Engineers, entitled, "The Periodicities and Damping Coefficients of Coupled Oscillators." This paper was printed in the Electrical Review, of New York; in abstract in the "Electrician" of London, in translation in l'Eclairage Electrique, and 10179
some years later appeared in translation in the Jahrbuch der Drachlose Telegraphie und Telephonie.

Another paper was read before the wireless Institute on "Resonance in the Secondary of a Transformer," or some such title; and later appeared, in part, in the Physical Review under the title "The Maximum Current in the Secondary of a Transformer."

Another paper was read by me before the Institute of Radio Engineers, about three years ago. entitled, "The Resistance of the Spark and its Effect upon the Electrical Oscillations of Electric Oscillators,'' or some such title. This paper appeared in the Proceedings of the Institute.

Last year I read a paper before the Institute on ''The Effect of the Spark upon the Oscillations of Electric Oscillators.'' This paper has been set up in type, but has not yet appeared in the Proceedings.

In addition to these papers, I have contributed several papers to the Electrical World and Engineer relating to the coupling coefficients of electrical oscillators, but I do not remember the dates, except that they are some
10181 years back.

Q. 323. Referring now to the Massachusetts tests of July 3rd and 4th, which you say you witnessed, what criticisms if any have you to make of such tests, and what conclusions did these tests lead you to? Please give your reasons for any opinion which you may express? A. The conclusion I reached from these tests was that they neither showed whether there was an oscillation of current through the spark gap or not, in the apparatus tested; also that the apparatus tested could scarcely with fairness be assumed to be a Kilbourne & Clark transmitter in its normal condition of operation. Therefore I concluded that these tests did not disclose the mode of
10182 operation of a Kilbourne & Clark set.

In the first place, I noted that the set under test was equipped with a generating source which the nameplate it bore indicated was a Marconi Wireless Telegraph Company's generator of four horsepower. This nameplate read,''Marconi Wireless Telegraph Co. of America, Lords Court Building, 27 William Street, New York. HP (blank). Volts 120-220. Speed 2300-2500. Amperes 9.1. KW. (The inscription here is illegible). No. 153,141. Patented March 12th and May 7th, 1901.''

I do not, of my own knowledge, know the characteristics of this dynamo, nor do I of my own knowledge know the characteristics of the Kilbourne & Clark generator designed to be used with this set; but it is a matter of common knowledge that the character of the dynamo employed with a given set is a matter of considerable importance in determining its precision of operation, and in testing a given transmitter it is inadmissible to substitute a different dynamo from the one with which the transmitter is designed to operate, unless the dynamo substituted has been carefully tested to see that it has substantially the same characteristics as the dynamo with which the transmitter is designed to operate. 10184

If, as I am informed, the Marconi dynamo in question has the characteristics more or less pronounced of a constant current machine, while the dynamo of the Kilbourne & Clark transmitter has more or less pronouncedly the characteristics of a constant potential machine, the substitution of the Marconi generator for the Kilbourne & Clark generator would invalidate the tests so far as these might be expected to reveal accurately the mode of operation of the Kilbourne & Clark transmitter.

Mr. Betts: Objection is made to the present answer on the ground that on its face it is merely based on information, the source of which information is not disclosed. 10185
Therefore the answer is objected to as hearsay and secondary.

A. (Continued) The distinction between a constant potential and a constant current dynamo is a very fundamental distinction. A constant current dynamo is one which tends to maintain the current that is developing constant irrespective of changes which may be taking place in the circuit to which it is supplying current, while a constant potential dynamo is one which tends to maintain its potential, or electrical pressure, constant irre-

10186 spective of changes which may take place in the circuit to which it is supplying current.

No dynamos are strictly constant current or strictly constant potential, but most dynamos belong either to one class or the other and are usually distinguished by reference to this particular characteristic of operation.

The tests were insufficient, therefore, in not making use of a Kilbourne & Clark dynamo instead of a Marconi dynamo, or else in not comprising a comparative test of the two dynamos to show that they were of the same character.

10187 That these two classes of dynamos are not interchangeable in their application is evident from the fact that the constant current dynamo only can directly supply an arc light circuit while a constant potential dynamo only can directly supply a current to an incandescent lamp circuit. It would be entirely impracticable to interchange these dynamos in these two systems of lighting; in fact, it would be disastrous to do so.

The next criticism I have to make of these tests so far as the mode of operation of the Kilbourne & Clark transmitter is concerned, relates to the spark gap. In the tests of July 3rd, the spark gaps gave evidence of being very much out of order, and these indications were borne out
10188 by the fact that when after the tests the gaps were opened, their surfaces were found to be very badly pitted and in fact so badly pitted in the case of two gaps as to cause their permanent short circuiting. Between these tests and the tests of July 4th, these gaps were resurfaced, that is to say, the roughnesses were all turned off in a lathe, and the surfaces smoothed. They were then reassembled and put in operation during the tests of July 4th, but I must note that on inspecting these gaps when they were still open and before they were reassembled, I noticed that large pieces of lavite insulating material

 10189

had been broken away, and that the spark gaps gave evidence of no longer being hermetically sealed, so as to completely exclude the ingress of oxygen to the sparking surfaces.

Mr. Chaffee and his assistants sought to remedy this latter defect by filling up the spaces through which air might otherwise enter the gap with sealing wax, but no attempt was made to replace the broken lavite within the gap. As a consequence of the foregoing the tests made on July 4th were begun with gaps, the sparking surfaces of which were bright silver in the presence of a considerable volume of air. These two facts would seriously 10190
prejudice the tests. In the first place, it is a well understood fact that a quenched gap set does not operate at full efficiency as long as the sparking surfaces are bright silver; that is to say, until the sparking surfaces have been formed, and the oxygen included within the gap has been used up in forming the characteristic dappled surface noticed in such spark gaps.

Transmitters of this type greatly increase their current output from that secured with the initially bright sparkling surface until the surface is formed and the oxygen consumed, and this increase in efficiency and output may amount to a very considerable percentage of the total efficiency and output of the test. The time required 10191
for this transformation of the set during the forming of the sparking surfaces and deoxidization of the included air in the gap depends upon the extent of the use of the set and naturally upon the amount of oxygen included within the gap. I do not know that there is any definite data upon the number of sparks required with a given transmitter. An examination of the sparking surfaces of these gaps was made during the July 4th tests just before the tests were made in which the abnormal condition of a disconnected antenna circuit were performed.

10292

My observation of these gaps at that time led me to conclude that the surfaces were not yet quite formed, and in view of the large amount of air included in the spark gaps where the lavite was broken away, I should judge that even if the gaps were maintained hermetically sealed by the sealing wax, nevertheless the time required to bring these gaps up to normally high efficiency in operation would be longer than normally with such gaps.

The gaps were again opened and submitted to inspection after the tests had been made of the set with the antenna disconnected, and the gaps then showed bright spots or areas on their surfaces, showing where arcing
10293 had taken place when the antenna was disconnected; that is to say, a very distinctly different marking on the spark gap where several oscillations had passed through the gap in a given spark as compared with where either a single oscillation or perhaps one or more complete oscillations had passed at other points during the previous tests.

It seems to me from the foregoing that at no time during these tests was the set operated with spark gaps in the normal working condition of the Kilbourne & Clark apparatus, and for this reason, if for no other, the tests should, I believe, be considered unconclusive as to
10294 the real mode of operation of the Kilbourne & Clark apparatus.

Referring now to the connections of the apparatus used when the inside electrostatic deflecting plates were used, in such tests only were oscillograms made in which the to and fro movements of the cathode stream were deflected laterally so as to place a curve. In these tests one of the operating condensers of the set of the transmitter was shunted by certain resistances in coils entirely foreign to the Kilbourne & Clark set in its normal mode of operation. I refer to coils R′, R², I′. The resistance of

3399

3400
Ex Binder T1

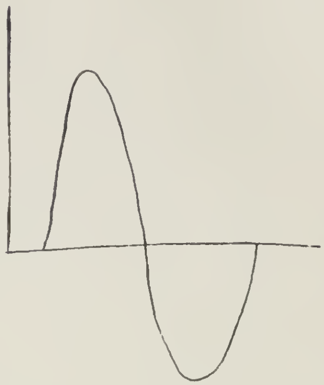


Fig 2

Stone Sketch

[illegible]grams 1. 2 + 3

 10201

R′ and R² were stated by those participating in the test, but the resistance or inductance of the coil *1′* I was unable to learn from them. It is difficult to say just what this shunted circuit would do in the way of disturbing the normal mode of operation of the set. It is practically impossible to calculate what this effect would be without a knowledge of the constance of the coil *1′*. Such effect also would depend upon a host of other conditions unknown so far as the apparatus of this test was concerned.

I come now to another source of uncertainty, and perhaps one of the most vital sources of uncertainty, as to
the significance which may be placed upon the observa- 10202
tions of these tests. I refer to the connections of the magnetic deflecting coils of the Braun tube with the spark gap circuit of the transmitter. This connection consisted of two leads of insulated wire placed as close together as practicable and each about eight feet long connecting the magnetic deflecting coils on the Braun tube, with a length of wire in the transmitter in series with the spark gap and marked S′ S². The magnetic deflecting coils connected in this way with the spark gap circuit cannot certainly indicate whether or not a single unidirectional impulse occurs in that circuit, or whether a complete electrical oscillation takes place therein. Assuming for
the sake of exposition that a single unidirectional im- 10203
pulse passes through the conductor S′ S² in series with the spark gap, as shown in Fig. 1 of the sketch I am now drawing (reproduced opposite), the electromotive force impressed upon the leads to the magnetic deflecting coils will be of the form shown in Fig. 2 of this sketch I am now drawing, because, owing to the extremely high frequency of the phenomena, the fall of potential or electrical pressure along that conductor will be proportional, almost exactly, to the rate of change of the current in the conductor.

Plff 4

Fig 1

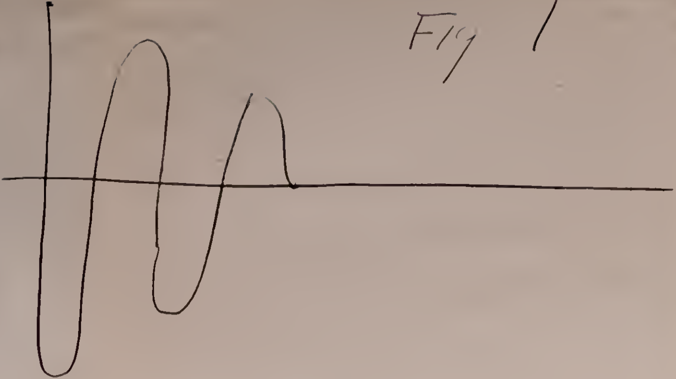
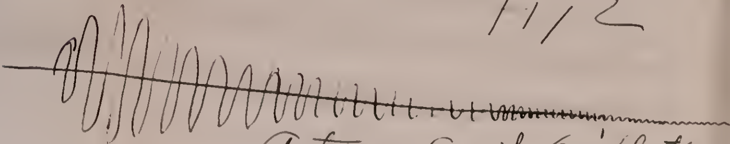
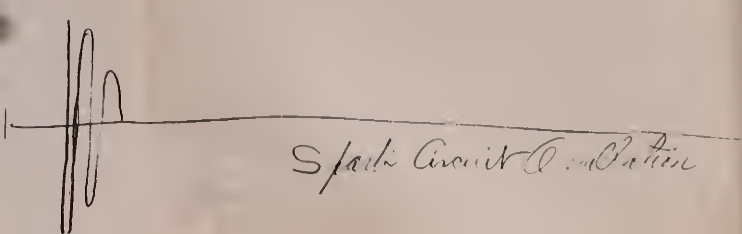


Fig 2



Antenna Circuit Oscillations

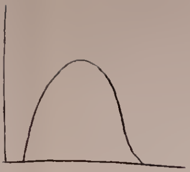


Spark Circuit Oscillation

3400

Defs Ex Bunder T1

Fig 1



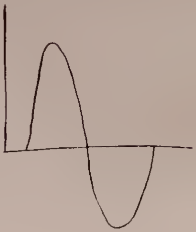
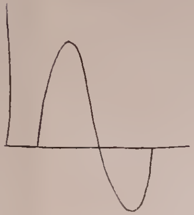


Fig 2

Fig 3



Defs Ex Stone Sketch

Diagrams 1. 2 + 3

10204

Now, if the resistance of the magnetic deflecting coils on the Braun tube, and the leads therefrom to the transmitter, be very large compared to the inductance of these single loops, coils and leads, then the deflection of the cathode ray beam in the Braun tube should be almost exactly that illustrated in Fig. 2 of my diagram because the current through such a circuit would consist of one complete oscillation, a positive followed by a negative oscillation. If, on the other hand, the inductance of the single lead magnetic deflecting coils, and the leads leading to the transmitter was very large compared to the resistance of this combination, then the deflection of the cathode
10205 beam and spot of light on this fluorescent stream would correspond very closely to the unidirectional impulse I have depicted in Fig. 1 of my sketch.

In the intermediate case, and probably the most likely case, where neither the inductance nor the resistance was enormously preponderating in this effect in its leads and coils, the current in these coils, and therefore the deflection of the cathode stream and spot of light, would correspond to the curve I am now drawing, Fig. 3, which resembles a complete oscillation somewhat heavily damped.

To sum up this last point it is merely a question of the
10206 ratio of the inductance to the resistance of the conductors constituting the deflecting coils on the Braun tube, and the leads therefrom to the transmitter, whether or not the indications of the Braun tube will portray a unidirectional impulse in the primary, or a damped complete oscillation of current in the primary, even though the real current in the primary be a single unidirectional impulse.

Mr. Betts: The answer is objected to as speculative and argumentative, and also I object to the sketch made by the witness in connection with the last answer.

 10207

Mr. Scull: The sketch made by the witness is offered in evidence.

Marked Defendant's Exhibit "Stone Sketch, Diagrams 1, 2 and 3". (Deft's Ex. Binder T.)

Mr. Betts: I object to the sketch as speculative, argumentative, and secondary, and therefore incompetent.

(Recess.)

(After recess.) 10208

John Stone Stone resumes the stand.

Direct examination by Mr. Scull: (continued)

A. to Q. 322 (resumed). Coming now to the oscillograms that were made of oscillations taking place in the aerial circuit, such as BK, BL, BM, my observation of the performance of the Braun tube and of the negatives that were taken representing these tests, convinced me that there was no indication in these of the time required for the energy to be transferred from the spark circuit to the aerial. This is true of the images that I observed on the
fluorescent screen, of the photographic negatives taken 10209
of these images on the fluorescent screen, and of the photographic plates made from the negatives. There is indeed a blur of the plate at the lefthand side of the image, which might be mistaken to represent the configuration of the oscillations, or at least, under misapprehension, be taken to give the envelope or contour of the oscillations; but I am satisfied that the true shape of this envelope or contour of the amplitudes of the oscillations at the beginning of the train of oscillations, could not be determined either from the image on the fluorescent screen,

from the negatives of the photographic plates, or from the prints taken from those negatives. It is indeed a fact that the rate of motion of the cathode beam transverse to its up and down motion of oscillation in these tests was so variable, and its total path was so short, as to completely distort the envelope or curve of amplitudes of the oscillations. Owing to some miscalculation on the part of Prof. Chaffee, the discharge of the condenser C'', which determined this lateral deflection of the cathode beam, was itself oscillatory instead of being unidirectional; so that the time axis, that is to say the axis at
10211 right angles to the image of the oscillations, not only came to rest before the oscillations ceased, but actually returned upon itself, with the result that it telescoped, so to speak, the long train of waves back towards the starting point, giving a heart shaped figure somewhat like Fig. 4 of the sketches I am now making, (reproduced opposite) instead of an elongated kite shaped figure such as Fig. 5 of my sketch. In spite of the enormous distortion resulting from the initial high speed of the spot of light along the horizontal axis, in the direction shown by the arrow in Fig. 4 on my sketch, its rapid diminution of speed to zero at the point *a* of that figure, and its reversal of direction and return
10212 towards the original position as indicated by the part of the curve I mark *b* in Fig. 4—I say, in spite of this frightful distortion, I am convinced that, had the fluorescent screen been sensitive enough, or the cathode rays powerful enough in these tests to show the envelope of the curve at the lefthand side of the figure, the resulting figure would have been not as shown in Fig. 3 of Plaintiff's Exhibit Cross diagram, but would have been more nearly like Fig. 6 of my sketch. Even in such a figure as I have shown in Fig. 6, it will be noticed that the rise of amplitude at the lefthand side of the figure is much more grad-

5405 Deft. Exh

Fig 4

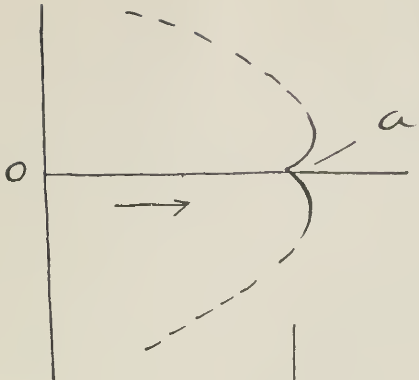


Fig 5

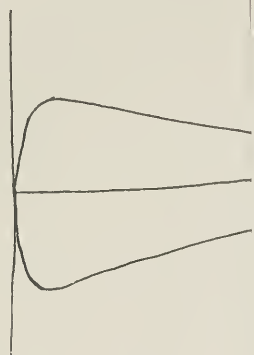
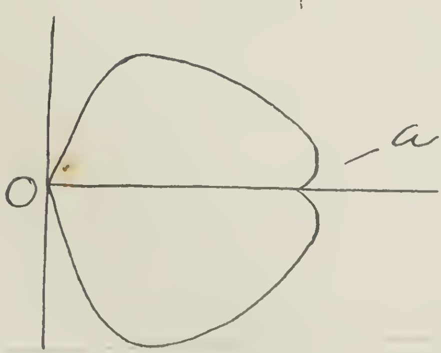


Fig 6



3405 Deft. Exh Binder T 2

Fig 4 o

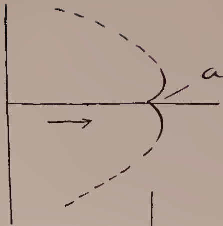


Fig 5

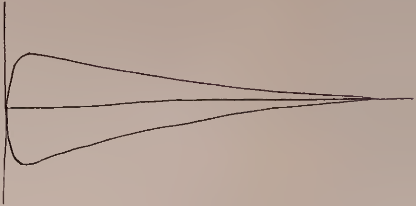
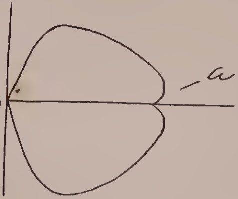


Fig 6 O



Def Ex Stone Sketch

Diagram 4, 5 & 6

 10219

ual than is the rise of amplitude in the case of the actual oscillations as I have shown them in Fig. 5 of my sketch, and this more gradual rise of amplitude is due to the fact that in the apparatus as set up by Prof. Chaffee, the velocity of the cathode ray is very much greater at that point than throughout the rest of its excursion between the points 00 and *a*; in other words, the wave train illustrated in Fig. 5 of my sketch has been elongated at the lefthand end and telescoped at the righthand end, owing to the method of deflecting the beam of the cathode rays employed by Prof. Chaffee. There is nothing in these photographs to my mind, to justify the assumption that 10220
the blur around the central line in the oscillograms of the aerial or dummy antenna circuit is indicative of a gradual rise of amplitude from its lefthand extremity, as shown in Fig 3 of Plaintiff's Exhibit Cross diagram (Vol. 5, p. 3118).

I conclude, therefore, that there is nothing in these diagrams to show whether there was more than a unidirectional impulse in the spark circuit or not.

> Mr. Scull: The last sketch made by the witness is offered in evidence as Defendant's Exhibit Stone Sketch Diagrams 4, 5 and 6. (Deft's Ex. Binder T2.) 10221
>
> Mr. Betts: Objected to as speculative and immaterial.

Q. 323. Did you observe Prof. Chaffee when he made the photographs of the oscillations, or the oscillograms and if so have you any criticism of the method which he pursued? A. I did observe the method pursued by Prof. Chaffee in making his photographic exposures. I noted that, particularly during the test of July 3rd, Prof. Chaffee often waited a considerable time, a minute or more, after

getting a visible oscillogram on the Braun tube, before he made his exposure; apparently waiting until he saw in the visible oscillogram just the condition that he wanted to reproduce in the photographic negative. In the test of July 4th Prof. Chaffee made his exposures much more promptly, though by no means indiscriminately of the photographic image in the Braun tube. On several occasions Prof. Chaffee made no photographic exposure whatever to perpetuate the oscillograph indications of the Braun tube. If the object were to determine whether or not it was possible, by the variations of adjustment of the apparatus being simultaneously
10223 made by Mr. Weagant, to momentarily produce an oscillatory current in the spark circuit, this discriminating choice of the moment at which to make an exposure by Prof. Chaffee would, in my opinion, be justified. But if the object were to determine the mode of operation of the apparatus broadly, the exposures should have been made indiscriminately, without reference to what the observer saw in the Braun tube.

Mr. Betts: The answer is objected to as argumentative.

Q. 324. At typed page 165 (Print, p. 3124) of the
10224 deposition of Prof. Cross he says:

> "I was well aware of the fact that the varying potential of external plates would not induce a correspondingly varying field within the tube, but that internal plates were necessary for such studies."

Other witnesses testifying for the plaintiff have made similar criticisms of the outside plates, such criticisms having been made by the witness Chaffee and others, Prof. Chaffee in effect saying that the deflection of the

 10225

cathode beam would only occur while the pótential of the external electrostatic deflection plates was varying. Do you agree with these statements, and please give your reasons for agreeing or disagreeing?

Mr. Betts: The question is objected to for the reason that it only purports to quote a single sentence of Prof. Cross's testimony, and further that it purports to summarize the testimony of the other witnesses. The whole testimony of the other witnesses should be considered.

Mr. Scull: It is noted that the witness has read all of the testimony referred to in the question, and it is wholly unnecessary to repeat here such testimony. 10226

A. I do not agree with the statements of Prof. Cross in regard to the applicability of the use of external plates for the purpose of getting accurate observations from a Braun tube, insofar as these remarks of Prof. Cross are not limited with regard to the frequency. It is quite true that there is a phenomenon in the Braun tube which modifies these indications when the variations of potential of the outside deflecting plates is slow, and that this produces a certain limitation in the use of outside plates with a Braun tube when slow and irregular fluctuations of potential are to be indicated. However, the perturbating of phenomenon is of such character that when the rapid changes or fluctuations that are occurring is as high as those occurring in radio telegraphy; that is to say, when the fluctuations of potential are alternating or pulsating and of a radio frequency, so-called, the perturbating phenomenon due to outside plates does not have an opportunity to assert itself. 10227

With regard to the contention that the deflection of the cathode beam only takes place while the potential difference of the outside deflecting plates is varying, I regard this statement as strictly inaccurate.

The phenomenon which causes the behavior of a Braun tube with outside plates to differ from this mode of operation under corresponding conditions with inside electrostatic deflecting plates has long been understood, and has been the subject of very careful investigations, notably by W. J. Milham in a doctor's dissertation at Strassburg in 1901, and this effect is summarized by Dr. Zenneck in his treatise entitled "Electrical Oscillations and Wireless Telegraphy."

10229 Mr. Betts: I object to reference to Dr. Zenneck's book as secondary evidence on the ground that Dr. Zenneck is alive and witnessed the tests in Cambridge, Mass., on July 3rd and 4th, and hence Dr. Zenneck is available to give primary evidence.

(Answer continued) I have not the original German edition of this book, but I have before me the French transaltion "Paris, Gauthier-Villars, 1908," Volume I. On pages 4 and 5, 6 and 7 occurs a description of the Braun tube and the method of its use for the determina-
10230 tion of the magnitude and extent of variations of magnetic and electric fields, the first being produced by magnetic deflecting coils outside the tube and the second being produced by electrostatic deflecting coils also outside the tube.

In speaking of the deflection of the cathode beam by outside electrostatic deflecting plates, on pages 5 and 6, the fact that the deflections of the cathode beam are not directly proportional to the electric field outside the tube in the case of slow variations is pointed out, and the exact mode of operation of this device is described. The author then proceeds as follows, at the top of page 7:

 10231

"The fact, that the deflection does not remain "constant and that the pencil of rays returns grad- "ually to its initial position is explained as fol- "lows: The electric field in the interior of the tube "is destroyed by degrees by the action of the ca- "thode rays. It is for this reason that the Braun "tube is not utilizable in the manner described at "5c for a constant electric field (8). If it had "to be utilized in such cases the glass diaphragm "D (here he refers to Fig. 3 on page 4) should be "replaced by a metallic diaphragm similar to "those employed for anodes. With such a tube "the deflection remains constant in a constant "field (9). 10232

"In the case of a variable electric field the "Braun tube as described in 5c is utilizable if the "variations of the field are sufficiently rapid so "that the return of the pencil of rays to its origi- "nal position can have no effect."

To be a little more explicit about this phenomenon, it actually consists in the gradual formation of a charge of electricity on the inside of the tube opposite to the charge on the electrode outside the tube and when this charge on the inside of the glass of the tube is sufficient to exactly neutralize the electric field of forces within the tube due to the outside plates, the cathode beam returns to its original position of no deflection. However, this phe- 10233
nomenon as appears upon its face, is a relatively slow process, and in the case of current varying with the enormous rapidity of alternations of, say, 500,000 times a second, this phenomenon is completely inoperative; in fact, outside plates with Braun tubes can be used for the production of oscillograms of alternating currents of such ordinary frequencies as 500 cycles a second, owing to the subordination of this source of perturbation due to the rapidity of the fluctuation of the potential of the plates.

10234

The numerals (8) and (9) refer to notes at the end of Volume 1, as follows:

> "(8) Consult for this: W. J. Milham on the "use of Braun tubes in the measurement of elec- "tric fields (dissertation Strassburg 1901) (Uber "die Wermendbarkeit der Braunschen Rohre zur "Messung elektrischer Felder). (9) See A. Weh- "nelt, Verhandl d. Physik. Ges. t. 65 1903, p. 29)."

Mr. Betts: I object to the quotation from Dr. Zenneck's book on the ground that it is fragmentary and the portion quoted and translated by the
10235 witness has been separated from its context.

Mr. Scull: Defendant offers in evidence pages 4, 5, 6 and 7 and page 478 of the French translation of the work of Dr. J. Zenneck, referred to by the witness.

Mr. Betts: Objected to as secondary evidence on the ground that Dr. J. Zenneck, the author, was present at the tests on July 3rd and 4th, and notice was given by the defendant that he would be called as a witness on its behalf, and hence these pages are merely secondary evidence, and not competent testimony.

10236 Mr. Scull: These pages are offered and have been used by the witness simply to corroborate what the witness has stated was known in the art at the time these tests were made.

Mr. Betts: The evidence is still secondary because Dr. Zenneck is the one who should give primary evidence in corroboration of the witness's testimony.

Q. 325. Please state the difference between an impulsive excitation of an antenna, where there is but one half complete oscillation through the spark gap, and where

 10237

there are two complete oscillations through the spark gap, and state whether in your opinion there is any material difference between the two?

Mr. Betts: Objected to as not within the provisions of the order of the Court under which this testimony is taken. It is obviously a purely expert question, and not in rebuttal to the tests made by the Plaintiff on July 3rd and 4th.

A. There is no material difference between an antenna excited in these two different ways. The advantage of 10238
an impulsive excitation consists primarily in conserving the energy which would otherwise be, to a very considerable extent, dissipated in the spark circuit. This dissipation of energy in the spark circuit, where a considerable number of alternations take place in the circuit, becomes very considerable, and this largely for the reason that the spark, which starts initially with a small resistance and therefore with a small energy absorbing power, rapidly increases in resistance with time, so that the energy absorbing power of the spark, after the first two or three oscillations, is much greater than it is initially. It follows from this, therefore, that in an impulsively excited system, where the oscillations through the spark 10239
have been reduced to the first two complete oscillations, during which the spark resistance is very low, there is little more energy loss to be avoided by a further shortening of this time of impulsive excitation. However, there is an advantage, a very slight advantage, nevertheless an advantage, in still further shortening the time of impulsive excitation from two complete oscillations to one half oscillation. It is scarcely, however, of a magnitude to be called material.

In illustrating the distinction between these two forms of impulse excitations, I produce a sketch

10240

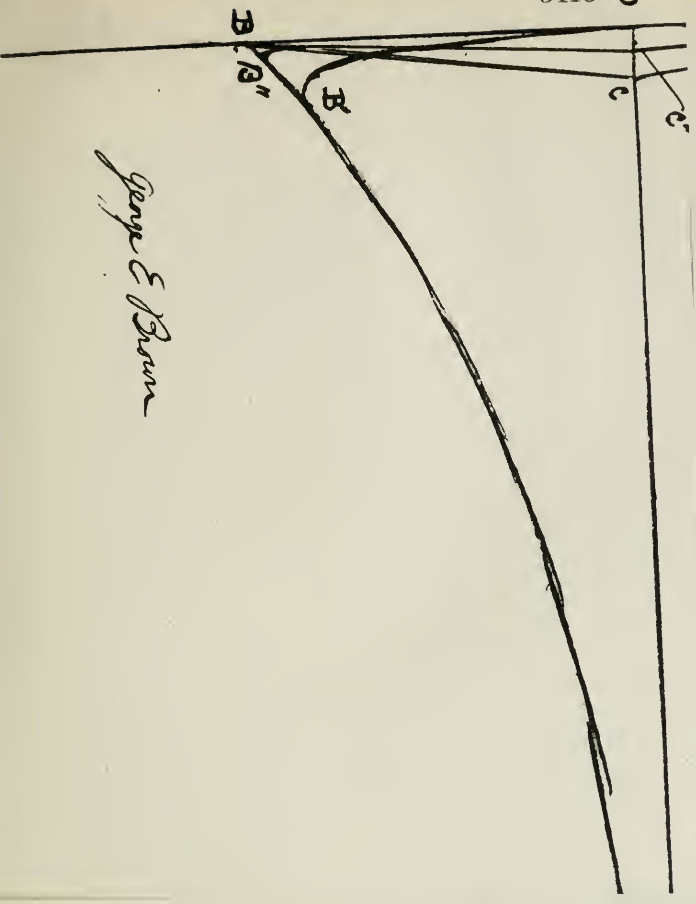
(reproduced opposite) showing the wave train in an antenna or dummy antenna having a decrement of 0.06, excited by impulse excitation. In this sketch the lines ACB represent the decrescence of amplitude of two complete oscillations in the spark circuit, the lines OA'DB'O represent the decrescence of oscillations in the antenna or dummy antenna circuit, corresponding to the two complete oscillations in the spark circuit. The lines AC'B represent the decrescence of oscillations in the spark circuit, in the case of one complete oscillation, and the lines OA″DB″O represent the corresponding de-
10241 crescence of oscillations in the antenna or dummy antenna in the case of one complete oscillation in the primary. This figure, which is only roughly calculated, but nevertheless far nearer accuracy than any of the drawings so far introduced representing these oscillations, gives, perhaps, more clearly than I can express in language, the smallness of the difference resulting in the case of an impulse excitation when the impulse consists of one or two complete oscillations in the spark circuit.

I have not shown the case of one half oscillation in the spark circuit, because it would be impracticable to do so on the scale of my drawing. The lines for it would practically correspond to the lines OADBO.

10242 Mr. Scull: The diagram referred to by the witness is offered in evidence as Defendant's Exhibit Stone Diagram No. 7.

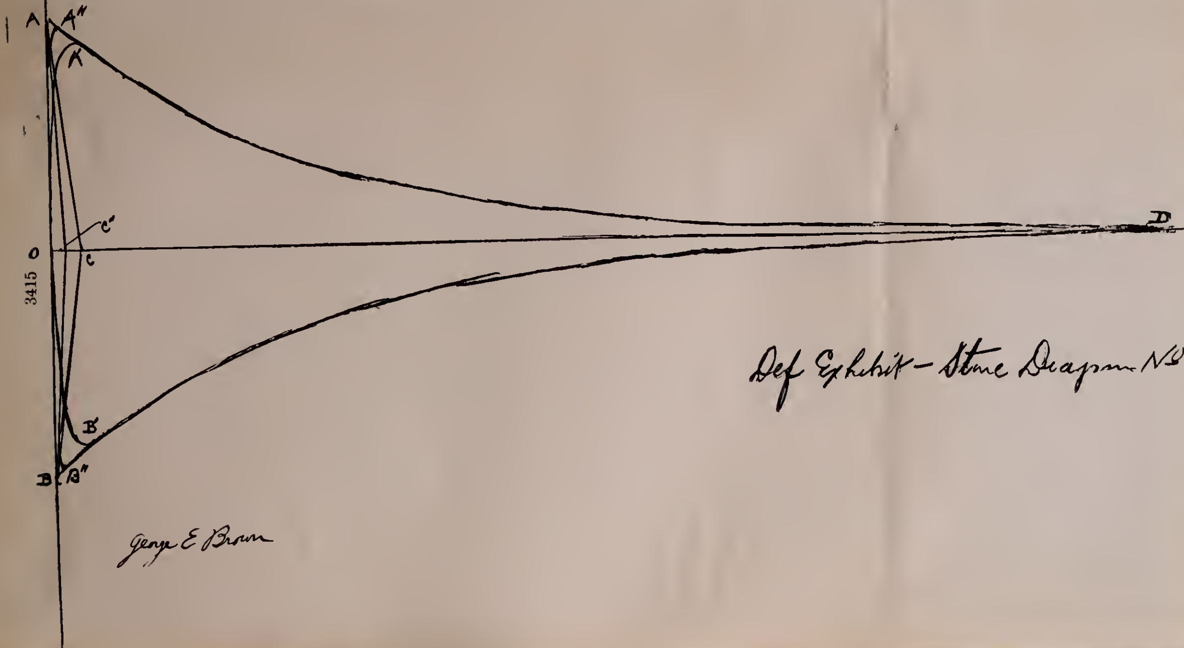
Mr. Betts: Objected to as immaterial, on the ground that Stone Diagram No. 7 is not connected to or associated with any given transmitter.

Q. 326. What is the difference between an impulsive excitation of an antenna and a loose-coupled persistent primary excitation of the antenna, and please state whether this difference, if any, is material, and if so, why?


O
B
B"
B'
C
C'
George E Brown

$\delta = 0.06$

Two complete oscillations through Gap-



Def Exhibit - Stone Diagram No 7

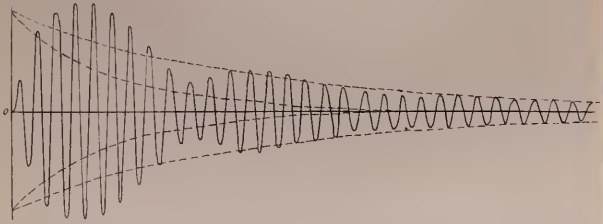
George E Brown

3417

3° Dans le cas de la figure 485, après chaque battement la phase de l'oscillation résultante change de 180°

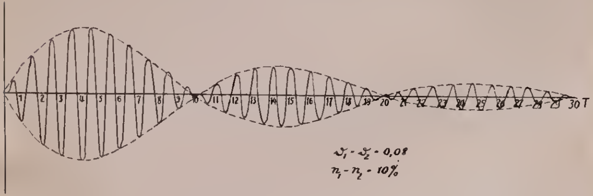
On voit immédiatement la raison de ces battements, en comparant

Fig. 484.



les oscillations propre et forcée de la figure 484. Au début, ces deux oscillations different de 180° dans la phase, donc s'affaiblissent; progressivement elles deviennent de même phase et se renforcent, et ainsi de suite.

Fig. 485.



III. — Courbes de résonance dans l'action d'un oscillateur sur un autre.

322. Généralitiés — Si l'on change la frequence de l'un des deux oscillateurs, les oscillations dans le secondaire changent aussi et les courbes de resonance se construisent comme à 310; elles montrent de

 10255

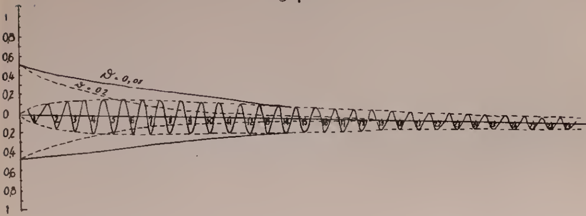
Mr. Betts: Objected to on the ground that this question is not within the order of the Court.

A. The impulsive excitation of an antenna is pretty well exemplified in my diagram, Stone Diagram No. 7, the characteristic of which to be specially noted is that the length of the wave train or oscillation train there represented in the antenna circuit, and its rate of decrescence, is practically uninfluenced by the rate of decrescence or length of wave train in the spark circuit. The persistence of oscillation depends in that case, as seen from that diagram, practically wholly on the constants 10256
of the antenna, and may be calculated from a knowledge of the decrement of the antenna alone.

In the case of a loose-coupled persistently oscillating primary transmitter system, a totally different state of affairs exists, which is best illustrated by having reference to Fig. 481, Page 86 of Volume 2 of the French translation of the Zenneck book, to which I have already referred. In this figure (reproduced page 3418) the antenna oscillations are depicted as gradually rising. The outermost lines of the diagram indicate the natural rate of decrescence of the persistently oscillating primary, while the inner dotted lines indicate the more rapid decrescence natural to the 10257
antenna system. It is characteristic of these transmitters that the rate of decrescence or dying out of the oscillations of the antenna system is controlled, not by the natural rate of decrescence of that circuit, that is to say, not by the decrement of that circuit, but by the rate of decrescence or decrement of the oscillations of the primary or driving circuit, so that even though the antenna circuit be naturally very highly damped, it will oscillate persistently under the influence of a loosely-coupled primary circuit of small decrement or rate of decrescence.

forcée. Ce n'est plus le cas, comme le montrent les figures 482 et 483. L'amplitude de l'oscillation forcée, pour laquelle on avait d'apres

Fig. 482.



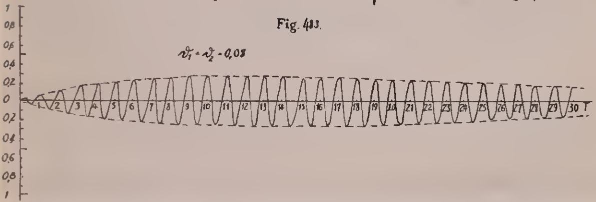
308 a et 319 b, dans le cas de l'isochronisme,

$$i_{10} = i_{20} = \frac{\mathcal{E}_{a_0}}{2\beta_2(\delta_1 - \delta_2)}, \tag{3}$$

$$\vartheta_{10} = \vartheta_{20} = \frac{\pi n \mathcal{E}_{a_0}}{2(\delta_1 - \delta_2)} \quad (^*), \tag{4}$$

est un des facteurs qui determinent l'amplitude maxima. Mais, en

Fig. 483.



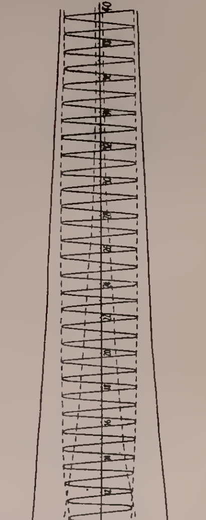
outre, celle-ci depend de la rapidité avec laquelle l'amplitude de l'oscillation croit et décroit. Tandis que pour l'amplitude de l'oscillation forcée la différence des facteurs d'amortissement intervient

(*) N'est pas valable pour $\delta_1 = \delta_2$; voir b.

Aft. Ex Bender T6

3418

Fig 481.



Marconi
Kilbourne & Clark Mfg Co

Pages 86 87 & 89 of French translation of Zenneck's book offered at typed page 47 of J Stone Stone's deposition
George E Brown (Rep).

10258

Fig. 481, to which I have referred, is not long enough to show more than a portion of the wave train or oscillation train in the antenna, but Fig. 482 on the following page, 87, illustrates another case of a transmitter with a very loosely-coupled persistently oscillating primary. Both of these cases show very loosely-coupled primaries and secondaries tuned to the same frequency. When, however, the frequencies of the two circuits are not exactly the same, or when the coupling between the primary and the secondary circuits is enough to slightly detune the circuits, the oscillations in the secondary are
10259 either of the character shown in Figs. 484 or 485 on Page 89 of Volume 2 of this French translation of the Zenneck book.

This difference to which I have referred between the two transmitters is very material, and for the reason that in the loose-coupled transmitter, energy is absorbed in the spark gap chiefly, but partially also in the condensers, without doing any good whatever, and the efficiency of the transmitter is thereby seriously limited. This absorption of energy is particularly baneful after the first two or three oscillations in the primary circuit, because by that time the resistance of the spark commences to run up and enhance the useless energy absorp-
10260 tion.

Mr. Betts: I object and protest against this answer as violation of the stipulation and order of the Court on which this testimony was taken, for it was clearly understood, I am sure, by the Court, that the testimony the Defendants were to take was to be solely in regard to the tests conducted by Dr. Chaffee at the Cruft high tension laboratory. The present question and answer is obviously a question asked of an expert on the question of infringement. I am helpless to stop

 10261

this examination as we are not before the Court, but I enter my vigorous protest.

I also object to the witness's reference to the book of Dr. Zenneck, on the ground that he was present and notice was given by the Defendant that they would call Dr. Zenneck as a witness; hence reference to this book is secondary evidence.

I also insist that the whole of both volumes of the book be offered in evidence, rather than mere scattered pages. I am willing to stipulate, however, that at the conclusion of the trial both volumes may be returned to the witness. 10262

Mr. Betts: I may further state to the Court that it is my understanding that there is an English translation of Dr. Zenneck's book entitled "Wireless Telegraphy" printed in 1915, and if the witness, or Counsel for the defendant, wishes to refer to Dr. Zenneck as an authority, it is submitted that he should refer to a book written in English rather than in French or German, which is a foreign tongue.

Mr. Scull: This witness has not referred to the Zenneck French translation as necessarily authoritative, but simply as a short and easy exposition of what this witness as well as others in the art 10263 knew at the time the tests in Massachusetts were made. This particularly applies to pages 4, 5, and 6, which were offered in evidence in connection with the preceding questions, and to which the objection and the statement that counsel would insist on having the whole book offered in evidence was not made.

As to the second volume, the witness has merely referred to certain figures appearing at pages 86 to 89 in lieu of drawing such figures himself as simple and easy and a time saving method of producing such drawings.

The objection now made by counsel requiring the production of the entire book was made after remarks were passed across the counsel table, indicating that the French translation used by this witness is his personal property, and one which he does not care to let go out of his possession particularly for such a long time as it might be tied up in connection with this litigation.

In reply to the last remark of Plaintiff's coun-
10265 sel it is noted that the English translation to which he refers is not one of the books to which the witness has referred, this latter book never having been translated into English.

In view of the fact that there is nothing in Figs. 481 to 485 which could not be reproduced by the witness, if he had time to do so, photographic copies of these pages are now offered in evidence and defendant's counsel declines to offer the whole of the two volumes. This declamation is subject to withdrawal in the event that it it found that in sufficient time for the trial a second copy of the French translation can be obtained.

10266 I offer in evidence photographic copies of pages 86 to 89 inclusive of the second volume of the French translation of the book by Dr. Zenneck, such pages showing Figs. 481, 482, 483, 484 and 485 referred to by the witness. (Deft's Ex. Binder T6.)

Mr. Betts: Same objection and also on the ground that these pages contain evidently words in the French language of which no translation has been produced.

Mr. Scull: Attention is again called to the fact

 10267

that these pages are produced not for the purpose of any wording thereon, but simply as an easy method of reproducing the Figs. shown thereon.

Q. 327. In Prof. Morecroft's deposition in this suit, he has said in effect that there should be no potential node in the aerial circuit of the Kilbourne & Clark or Simpson transmitter, because of the unequal absorption of energy in the two parts of that circuit on either side of the alleged nodal point. Do you agree with this?

Mr. Betts: I object to the question if it is sought to elicit from the witness a disagreement 10268
with Prof. Morecroft on the ground that the testimony of Prof. Morecroft was adduced by defendant's counsel in cross examination, and they thereby made Prof. Morecroft their own witness and cannot contradict him.

A. I consider that the unequal absorption of energy on either side of the nodal point would not preclude the development of a node of potential at that point in the sense in which a node of potential would naturally be understood by practical engineers. It is a fact that an absolute node in the mathematical sense of absolute zero potential could not naturally occur between these two 10269
halves of the system where the absorption of energy was unequal in each, but the approximation in any practical case particularly where the decrement is as low as .06 or .08 would be so close that it would probably be impossible to detect, except by the most delicate indicating means, the potential at such a point due solely to the unequal absorption of energy of the system on the two sides of the node. There is probably no case in physics of an absolute node and yet we do not hesitate to speak of nodes in vibrating strings and other vibrating bodies.

I consider the criticism hypercritical.

Q. 328. If in making the Massachusetts tests Dr. Chaffee had made a test of the alleged Kilbourne & Clark apparatus with a rectifier in series with a spark gap to compel a unidirectional single impulse excitation, what effect would this have had upon the tests and the conclusions to be drawn therefrom?

Mr. Betts: Objected to as speculative.

A. Such a test would have determined whether or not
10271 the method of testing used in the Massachusetts tests was competent to determine whether the excitation was a single unidirectional impulse or two or more complete oscillations.

Cross examination by Mr. Betts:

XQ. 329. Have you been retained by the Kilbourne & Clark Company to testify in this case? A. I have not.

XQ. 330. During the examination of Messrs. Chaffee, Morecroft, Coffin and Cross at the Cruft High Tension Laboratory, did you assist Mr. Farnsworth in the cross-examination of any of the witnesses by preparing questions or suggesting cross questions? A. I think that on
10272 several occasions I told Mr. Farnsworth whether or not I agreed with the statement of the witnesses, but I am not sure that I formulated any questions, but I may have.

XQ. 331. Did the Kilbourne & Clark Company pay you for your time and expense in coming to Cambridge? A. They have not so far, but I expect to send them a bill for my services.

XQ. 332. When first did you see a Kilbourne & Clark Simpson mercury valve transmitter? A. In the Cruft laboratory.

XQ. 333. July 3rd? A. July 3rd.

 10273

XQ. 334. Whose note book did you use in your direct examination in giving the nameplates on the motor generator? A. I used my own note book.

XQ. 335. In whose handwriting were the notes? A. The copy of the items on the nameplate are in the handwriting of Dr. Zenneck to whom I handed my notebook as he had no paper on which to make a record.

XQ. 336. Did you ever examine spark gaps of the Kilbourne & Clark mercury valve transmitter before July 3rd, 1916? A. Never.

XQ. 337. While you were at Cambridge last week attending the tests, and the evidence and Messrs. Chaffee, 10274
Morecroft, Coffin and Cross, did you examine the negatives of the tests of July 3rd and 4th? A. I think I examined all the negatives that were made during those tests and some negatives that were there present that had been made from tests prior to the July 3rd and 4th tests. I may have overlooked a few of the negatives of the July 3rd and 4th tests, but I saw several negatives of each type of test made each day.

XQ. 338. And prior to your giving your present deposition, did you hear, or have you read the evidence of Messrs. Coffin, Morecroft, Chaffee and Cross? A. I heard most of the testimony given, and read that part of the testimony of Prof. Coffin, and Prof. Cross which I did not 10275
hear.

XQ. 339. Prior to your giving your present deposition did you read the testimony of Messrs. Simpson, Greaves and Kolster, regarding the tests of the Braun tube at Washington University at Seattle? A. I did not.

XQ. 340. During the tests of July 3rd and 4th at the Cruft High Tension Laboratory at Cambridge, Mass., did you observe what was shown on the fluorescent screen during these tests? A. I observed the screen during a large number of the tests. There were so many experts present that it was not always possible to get near enough to the tube to get a good view, but I saw one or two fluorescent screen pictures characteristic of each of the different types of tests made at that time.

XQ. 341. You have before you on the table, have you not, a copy of the English translation of Dr. Zenneck's book entitled "Wireless Telegraphy" published in 1915? A. Yes.

Q. 342. Do you find in this English translation of this book of Dr. Zennick entitled, "Wireless Telegraphy" any corresponding figures or descriptions to that contained
10277 on Pages 4, 5, 6, and Page 478 of Volume 1 of the French edition of Dr. Zenneck's book? A. I glanced through the English translation of Dr. Zennick's second book for such a description of the mode of operation of the Braun tube as I found in the French translation of his first book before referring to the French book for this description, as I should have preferred to have made use of the English translation of his second book, but I do not find the same material. This second work entitled, "Wireless Telegraphy" is a very much smaller book than Dr. Zenneck's original work "Electrical Oscillations and Wireless Telegraphy" and contains only a fragment of the detailed descriptions contained in the original work.

10278 Q. 343. You have used a phrase "excited by impulsive excitation" as applied to a wireless transmitter. What do you mean by that phrase? A. I meant by that phrase a transmitter in which the oscillations are set up in the antenna under the action of a spark circuit in which only a very small number of half oscillations, or alternations, occur.

Q. 344. How few half oscillations must there be set up in a spark circuit in order that there shall be, in your judgment, an impulse excitation transmitter? A. The question of the number of complete or half oscillations

 10279

in the spark circuit, or exciting circuit, has been defined by the committee of standards of the Institute of Radio Engineers and I do not remember haw many half oscillations or complete oscillations were there contemplated, but I should consider a transmitter in which there were seven or eight complete oscillations in the primary very distinctly an impulse excitation transmitter, and generally speaking any transmitter in which the number of oscillations in the spark circuit is so small that the rate of decay of the oscillations in the antenna is determined almost wholly by the decrement of the antenna
is an impulse excited transmitter in contradistinction to 10280
a transmitter in which the number of oscillations in the spark circuit is sufficiently large so that the rate of decay of the oscillations in the antenna will be largely influenced, or be largely characteristic of the decrement of the primary.

Q. 345. And any transmitter having more than eight complete oscillations in the spark circuit you would not call an impulse excitation transmitter? A. No; the number of oscillations which may take place through the spark in a transmitter while still maintaining the character of impulse excitation would depend very largely upon the decrement of the antenna. If the decrement of
the antenna were extremely high the transmitter with 10281
eight complete oscillations in the primary might naturally become intermediate in its action between an impulse excitation transmitter and a persistent primary transmitter. The characteristic of the impulse excited transmitter which differentiates it from the persistent primary excitation transmitter is the relative rates of decay of the oscillations of the two circuits. Thus a transmitter in which the oscillations in the spark die out in a very small fraction of the time during which the antenna is oscillating necessarily is of the impulse excitation type

10282

while a transmitter in which the oscillations of the primary persist almost as long, or quite as long, as the oscillations in the antenna, is characteristically a persistent primary transmitter. Now, when I spoke of eight oscillations I had in mind the fact that few, if any, practical transmitters would ever have an antenna with such a great decrement that the oscillations in the antenna would die out at such a rapid rate as to give only eight actual oscillations in the antenna.

Q. 346. Is it a fact that in all radio transmitters with which you are familiar having a spark gap in the primary circuit that the oscillations in the primary circuit tend to die out much faster than in the antenna circuit; that is to say, that the decrement of the primary circuit is always much higher than the decrement of the antenna circuit? A. I think in most practical cases the natural decrement of the primary has been higher than the natural decrement of the secondary, but through the interaction between these circuits when used as persistently oscillating primary transmitters the oscillations in the secondary are damped through a flow of energy back into the primary so that the damping of the secondary is no longer its natural damping but is a very much exalted damping due to the energy it loses
10284 not by its own resistance in radiation but by returning it to the primary and maintaining the spark in that circuit long after the spark in that circuit would normally have died out.

Q. 347. From what transmitter did you obtain the data which you have figured in Defendant's Exhibit "Stone Diagram No. 7."? A. No particular transmitter, that diagram being applicable to any transmitter impulsively excited by not more than two complete oscillations in the exciting circuit, or spark gap, and having a natural decrement in the antenna circuit of

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0.06. This corresponds very closely to the conditions of the damping antenna used in the Massachusetts tests. The decrement of that antenna from the data supplied to me by Prof. Chaffee and Mr. Weagant leads to approximately a decrement of .06 unless an excessive resistance wholly unwarranted in practice occurred somewhere in the damping antenna to ground in that apparatus.

I have no reason to believe that there was any such excessive resistance at any point in that circuit during those tests so I feel that this diagram "Stone Diagram
No. 7" is fairly representative of what might be ex- 10286
pected of the damping antenna of the Massachusetts tests, or of a real antenna corresponding to this damping antenna.

XQ. 348. I believe you have referred in your direct examination to a potential node. What is a potential node? A. A potential node is a point in an electrical vibrating system at which the potential remains zero or approximately zero while the rest of the system executes vibratory or pulsating differences of potential.

XQ. 349. In a working antenna of a wireless transmitter, all of the energy must flow across the so-called nodal point? A. In electrical oscillations along an ex-
tended conductor as in the case of an antenna, the energy 10287
does not reside in the conductor; the energy resides in electric and magnetic fields, wholly or almost wholly without the conductors. It is an archaic idea to speak of the energy flowing along a conductor; in a conductor, or through a conductor. As a matter of fact the conductor is merely the guide of electric phenomena in the form of magnetic or electric fields entirely outside the conductor, and the only function that the conductor plays is that of guide. The conductor, incidentally, absorbs a certain amount of this energy from the field, convert-

ing it into heat, through the resistance of the conductor, and it is these manifestations of the energy in the conductor that have so long led the practitioner to persist in regarding the energy as in the conductor, instead of, as a matter of fact, being in the field, without the conductor. This proposition is so general that even in the case of alternating currents traveling along telephone wires, the energy is conveyed wholly, or practically wholly, in the electric and magnetic field surrounding the wire, which travels with the velocity of light, and the only function served by the conductors is to guide that
10289 energy from the transmitting dynamo to the receiving motor or telephone.

In consequence of these facts, there is really no difficulty in conceiving of the node of potential along a conductor, even though there be unequal absorption of energy either side of the node; with the sole understanding, however, that the node, under these circumstances cannot be an absolute node but only an approximate node. There would be some fluctuations of potential at that point, and these fluctuations of potential would be extremely minute except in the case of enormous decrements, that is to say decrements of the order of tenths instead of hundredths, and where the absorption of
10290 energy is practically wholly on one side of the node.

XQ. 350. You have on your direct examination referred to the fact that you have taken out a considerable number of patents for wireless telegraphy. I show you printed Patent Office copies of United States Letters Patent No. 714,832, issued to J. S. Stone, dated December 2, 1902, No. 767,975, dated August 16, 1904 to J. S. Stone and No. 714,833, dated December 2, 1902, to J. S. Stone. Are you the John Stone Stone to whom issued those patents? A. I am.

Mr. Betts: I offer in evidence printed copies of the Stone patents Nos. 714,832, 714,833, and

 10291

767,975, and ask that they be marked Plaintiff's Exhibits Stone Patents, with the respective numbers.

Re-direct Examination by Mr. Scull:

RDQ. 351. I will ask you to read into the record the first paragraph on page v of the small book by Zenneck on Wireless Telegraphy referred to in your cross examination, under the title, "Extract from Author's Preface to the First Edition," which page is dated at the bottom December 1908.
A. (Reading) 10292

> "This book was written at the suggestion of "the Publisher, Dr. Enke. It was originally in-"tended to be an abridged form of my larger book, "'Elektromagnetische Schwingungen und draht-"lose Telegraphie' (Stuttgart, 1905). It has, "however, developed into something quite differ-"ent; evidence of this lies in the fact that only 79 "of the 332 illustrations of the larger book have "been reproduced here."

RDQ. 352. I will also ask you to read from the first page, entitled "Advertisement," the first sentence from the French translation of Dr. Zenneck's book, "Electromagnetic Oscillations and Wireless Telegraphy," from 10293
which you read in your deposition? A. (Translating):

> "The French edition of *Electromagnetic Os-"cillations* by Dr. Zenneck is a practically literal "translation of the German Edition of 1905."

Recross examination by Mr. Betts:

RXQ. 353. Will you also read into the record the whole of the Author's preface to the second edition of the book entitled "Wireless Telegraphy," by Prof. Zenneck, appearing at pages vii and viii? A. (Reading):

"Author's preface to the second edition. Only "two and a half years after the appearance of "the first edition, a second one has become neces- "sary, even though a French edition had already "appeared in the meantime. The book, therefore, "has been accorded a much more favorable recep- "tion than I had dared hope.

"This has served particularly to spur me on "to do everything within my power to make the "second edition representative of the present sta- "tus of wireless telegraphy. Due to its rapid de- "velopment, this meant an extensive revision of "the entire book.

10295 "Unfortunately I found it impossible to carry "out this revision without extending the scope. "In view of this wider scope, the book has been "renamed 'Text book' (Lehrbuch) instead of "'Elements' ('Leitfaden') 'of wireless teleg- "'raphy.'"

"In choosing my subject matter, I was guided "chiefly by the standpoint of the physicist. I have "frequently discussed arrangements or devices in- "volving a new physical idea, even though know- "ing that they had either not been used to date or "are no longer used in practice. To confine our- "selves to what is of practical importance will "only be proper when once it has been fixed what "really is of 'practical importance.' On this point,
10296 "however, the views of experts have changed very "rapidly during recent years; even today indi- "vidual views diverge widely and seem to be in- "fluenced less by scientific reasons than by patent "rights.

"Unquestionably, theoretical investigation, "laboratory experiments, and experiences in prac- "tice have cleared much in recent years. Never- "theless, there still remain a number of problems "which find no answer in the results obtained to "date. If then my presentation of these problems "falls short of the necessary clearness, the fault "does not rest entirely with me.

"In this edition, as in the first, I have received "friendly cooperation from many sources: from

 10297

"Dr. L. W. Austin (Washington, D. C.), H. Boas "(Berlin), Dr. L. Cohen (Brant Rock), F. Ducretet and E. Roger (Paris), Dr. Erskine-Murray "(London), the Gesellschaft fur drahtlose Telegraphie (Telefunken Co., Berlin), Dr. E. Huth "(Berlin), the C. Lorenz Co. (Berlin), the Marconi Wireless Telegraph Co. (London), Dr. E. "Nasper (Berlin), Dr. E. H. Riegger and Dr. Rukop (Danzig), Dr. G. Seibt (Berlin), The Societe "francaise de radioelectrique (Paris), and Prof. "C. Tissot (Brest). To all these I herewith express my thanks.

"Particular thanks are due Dr. A. Meissner "(Berlin), Prof. Vollmer (Jena), and Prof. M. 10298
"Wien (Jena). These have gone to the great trouble of reading through the entire proof, and by "their valuable advice have guided me against "many errors and defects.

"Lastly I thank the publisher, Dr. A. Enke "(Stuttgart) for the kind interest he has evidenced in the preparation of the book in its final "form.

J. Zenneck.

"Danzig-Langfuhr, Physikalisches Institut
"der Technischen Hochschule,
"Nov. 1912."

(Sgnd) John Stone Stone.

It is stipulated between the counsel for the respective parties hereto that the certificate of the notary and the signatures of the witnesses to these depositions are hereby waived. 10299

Mr. Farnsworth: I have just this moment (4.50) received a telegram from Mr. Skeel, in Seattle, as follows:

"Please notify Betts Defendant's surrebuttal "receiver tests will be made Monday, July 17th

10300

"at 9 A. M. at Kilbourne & Clark laboratory. "Assessors will be invited. I will apply before "Judge Neterer to make these tests official inter-"partes tests. If Mr. Betts cannot be here on "that day, have him advise me. By having tests "Monday the time of the Court will be saved."

Mr. Betts: I decline to accept notice of any tests on Monday, July 17th, on the ground first that no order of the Court has been obtained after hearing counsel allowing the Defendant to take any alleged surrebuttal receiver tests, on Monday, July 17th, or at any other time. Until this matter
10301 has been presented to the Court, Plaintiff will not attend.

Greenleaf Whittier Pickard, a witness called on behalf of the Defendant, being duly sworn, testifies as follows:

Direct examination by Mr. Farnsworth:

Q. You are the same Greenleaf Whittier Pickard who has heretofore testified in this case? A. I am.

10302 Q. You witnessed the Plaintiff's Massachusetts tests of July 3rd and 4th last and heard or have read the testimony of Plaintiff's witnesses with respect thereto on July 5th to 8th inclusive? A. I did.

Q. State briefly your opinion as to the validity of said Massachusetts tests and the conclusiveness of the results obtained; please give your reasons at this time, very briefly? A. In my opinion, the tests conducted at the Cruft laboratory were not in any way conclusive. The apparatus tested was not a Simpson mercury valve transmitter in its normal operating condition, nor were the arrangements of circuit and apparatus used in these tests such as to give correct results.

The Simpson mercury valve transmitter gaps were, according to my inspection, in a decidedly abnormal condition, apparently as the result of some past use or abuse; and the rest of the transmitter even including the motor generator was also abnormal. In the first place, a generator different from that employed by the Kilbourne & Clark Company was used to supply alternating current to the step up transformer of the Simpson mercury valve transmitter. Secondly, so far as my observation went, no attempt was made by Dr. Chaffee or any of his as- 10304
sistants to properly adjust or attach the trigger circuit at the nodal point; a very important adjustment in this transmitter.

With regard to the testing arrangements, that is to say the Braun tube and its connections, I was surprised to find a number of arrangements or conditions which in my opinion would preclude the possibility of the Braun tube giving a correct representation of the electrical phenomena in the transmitter.

I first observed that in the tests on July 3rd and 4th, long leads were used to connect the deflecting plates or coils, or both, with the transmitter. These long leads con-
sisted of pairs of twisted conductors about eight feet long, 10305
in which the conductors were so closely parallel—in fact only separated by the thickness of the insulation on the wire—that a very considerable electrostatic capacity was introduced at this point. Further, the deflecting coils of the Braun tube, after running through these long leads, were connected in shunt to a short length of the lead adjoining the spark gap with the condenser of the transmitter. This mode of connection, even in the absence of such long leads, would preclude a correct representation on the screen of the Braun tube, of the actual current

10306 passing through the trigger of the Simpson mercury valve transmitter.

Further, all of the tests which I witnessed were made, not on any actual antenna but on a dummy. This dummy consisted of a pair of Leyden jars, a resistance and an inductance. I observed that during all of the tests the jars comprising the capacity of this dummy brushed badly; that is to say, a very marked luminous discharge ran up along the glass from both the inner and the outer coatings of the jars. I have had a great deal of experience in testing transmitters on both actual and dummy
10307 antennae, and I have always found that when using a dummy antenna with a condenser in the form of a Leyden jar, in air, the brush discharge along the edges of such a condenser so varies the capacity and the resistance losses in the jar as to seriously affect any deductions which may be made from the normal capacity and resistance values of such jars. In other words, the values given by Plaintiff's witnesses of the capacity and resistance of this dummy aerial had little meaning, because of this brush discharge, which acted to considerably increase both the capacity and the resistance losses in the dummy antenna. When using an actual antenna, such brush discharges do not occur unless the potential of the circuit is raised to
10308 very abnormal values, and the results obtained are then consistent.

One simple way in which it may be determined whether the transmitter is or is not properly operative, is by listening to the tone of the spark. I am very familiar, by thousands of observations, with the normal tone of transmitters operating with five hundred cycle supply, and in particular I am familiar by actual observation with the normal spark tone of the Simpson mercury valve transmitter. During the tests of July 3rd and 4th, I listened in at least a score of times, on a wave meter inductively

 10309

associated with the dummy antenna, and in no instance
did I hear a spark tone which I should consider that of
the Simpson mercury valve transmitter when normally
operating. Owing apparently to a combination of im-
properly chosen alternating current generator, an incor-
rect adjustment of the trigger circuit with respect to the
nodal points, and an abnormal condition of spark gap,
the spark tones which I heard indicated the presence of
many partial discharges. This was confirmed by my ob-
servation of the rotating neon tube or stroboscope. In-
stead of showing very sharply defined images of the nar-
row portion of the tube, the effect, as I observed it, was 10310
that of a series of rather broad, ill-defined lines, which
indicated to me the presence of partial discharges close
to the initial discharge. Even if the spark gap had been
in normal condition as to surfaces, separation and air
tightness, the presence of such partial discharges would
have seriously interfered with its action.

Adjourned to Tuesday, July 11th, 1916, at 10.00 A. M.

New York, July 11th, 1916, 10.00 A. M.

TRIAL RESUMED. 10311

Greenleaf Whittier Pickard resumes the stand.

Direct examination by Mr. Farnsworth (continued):

Q. State first what you know and how you learned it, as to the constitution of the Simpson mercury valve transmitter, particularly as used in normal commercial service; and then state the facts showing wherein the apparatus tested by the Plaintiff in Massachusetts, pur-

10312

porting to be a Simpson mercury valve transmitter, was not in fact such, as you have above testified briefly? A. I first became acquainted with the Simpson mercury valve transmitter some time in February of this year, at the Bureau of Standards, Washington, D. C. This Simpson mercury valve transmitter was then under test by the Bureau of Standards, and I made a very careful examination of the transmitter, its connections and its adjustments. A little later, some time early in March, 1916, I again saw a Simpson mercury valve transmitter, this time in the laboratory of the Kilbourne & Clarke
10313 Manufacturing Company in Seattle. I spent a great deal of time with this transmitter and became thoroughly familiar with its normal adjustments and operation

From my knowledge of the Simpson mercury valve transmitter thus gained, and from my observations at the Cruft laboratory on July 3rd and 4th, I can positively state that the Simpson mercury valve transmitter, as used in these tests, was not in normal operating adjustment and was not operating normally. The facts that I observed and which lead me to this conclusion are as follows:

The alternating current generator supplying power to the transmitter was, as I observed, a five hundred cycle
10314 Crocker-Wheeler machine having a stationary field winding and a rotating armature. The nameplate attached to this machine indicated that it was built for the Marconi Company. I have not the slightest reason to suppose that this generator was in any way suitable for use with a Simpson mercury valve transmitter; in fact, my experience has shown me the contrary. I have always found it of great importance, I might say of controlling importance, to either proportion the generator to the transmitter or the transmitter to the generator.

The reason why such proportioning is essential is

briefly as follows. In order to obtain a good spark tone, that is to say, a regular recurrence of evenly spaced discharges, it is essential that there should be no partial or secondary discharges. To prevent this it is necessary that the synchronous impedance of the generator should be such that, with reference to the charactertistics of the transformer employed and the size of the transmitter condenser, there should be no building up of potential across this condenser immediately after a discharge. To attain this end, as I have above stated, very careful proportioning is necessary. Operating a radio transmitter
from an alternating current generator is a very different 10316
thing from running a bank of lamps. If one wishes to light the bank of lamps, it matters very little what the characteristics of the generator supplying current to the lamps may be; with a radio transmitter the conditions are quite different.

Dr. Chaffee has stated that it made no difference whether the spark tone was good or poor, so far as the high frequency phenomena in the transmitter were concerned. I disagree absolutely with Dr. Chaffee as to this, because both theory and my experience have shown to the contrary. Whenever partial discharges occur to any material extent, they tend to overheat the spark gap
and to prevent its proper action, that is to say, prevent 10317
its quenching; indeed, Dr. Chaffee himself has pointed out this fact on type Page 44 of his testimony. In discussing certain of the photographs taken of the Braun tube screen, Dr. Chaffee says,

> "Both of these photographs show the phe-
> "nomenon of beats in the oscillations of current
> "in the primary circuit. There are probably five
> "or six complete oscillations.
> "For some time the presence of these beats
> "troubled me, because the apparatus had not been
> "changed in its adjustment. The reason for the

10318

> "appearance of these beats was due to the fact "that the gap was hot."

The presence of partial discharges, therefore, does have a most marked and important effect upon the operation of the gap, and all of the high frequency phenomena occurring in the transmitter, and unless these partial discharges can be either eliminated or else made infrequent it cannot be said that the set is operating in its normal way.

Not only was the generator employed in these tests
10319 at the Cruft's laboratory one which in my opinion is unsuitable, but the spark gaps in the Simpson mercury valve transmitter were in very bad shape. Whether this impaired condition of the spark gaps resulted from the use of an improper generator with the transmitter, or from the improper adjustment of the transmitter itself, or from both, I cannot state. I was not present at any of the preliminary tests prior to July 3rd, when in all probability the damage to the gaps resulted. During the tests of July 3rd, as I have before stated, I observed both the stroboscope and listened in on the wave meter, and both of these instruments showed the presence of partial discharges, this being evidenced by the broaden-
10320 ing of the lines in the stroboscope and the lack of clear tone in the telephones attached to the wave meter. I suspected heating and possible injury to the gaps at the time, and when on July 4th I saw one of the open gaps my suspicions were confirmed. The surfaces were indeed very badly pitted; in fact, I do not know that I have ever seen a more marked case of injury to sparking surfaces than one of these gaps showed. Even after a very considerable thickness of silver had been removed (by turning off in a lathe), a large depression or crater still existed in one of the plates.

 10321

After the gaps were opened and after the surfaces had been machined off, they were again assembled and a short run made, perhaps an hour, when they were again opened for inspection. At this second opening, which also took place on July 4th, I observed that the surfaces showed rather marked oxidation; in fact, much more oxidation than I should have been led to expect from the normal amount of air contained in these spark gaps. The spark gaps used in the Simpson mercury valve transmitter, when they are in their normal condition, have something less than one cubic inch of air sealed up
in the space between the plates and in the thin annular 10322
ring surrounding the sparking surfaces. This corresponds with the presence of about three or four hundredths of a grain of oxygen in this space; an amount which would produce very little silver oxid on the plates, much less in fact than I observed when the gaps were opened for the second time on July 4th. I am therefore led to the conclusion that air must have leaked into these gaps, and I am confirmed in this by the fact that when the gaps were first opened, portions of the lavite insulating gasket were broken away, so that the air seal was destroyed. It is true that an attempt was made to reseal these gaps by smearing sealing wax on the broken part
of the lavite gasket. I do not believe, however, that any- 10323
one in their sober senses would maintain that sealing wax is suitable material, either from a mechanical or a thermo standpoint, to replace such a material as lavite, and I do not consider that this attempted repair of the gap actually placed it in anything like normal operative condition.

Finally, the adjustments made by Messrs. Chaffee and Weagant were not in my opinion normal adjustments.

On typed page 92 (Print, page 3050) of his testimony herein Dr. Chaffee was asked whether he had made sure that the point of connection of the spark gap to the antenna was at a potential node. He answered as follows:

> "I varied the point of contact between the "spark gap circuit and the dummy antenna "throughout the range possible with the set, and "adjusted it to the position of best operation. I "do not think it is possible to have a potential "node within the range through which it is pos-"sible to place this junction."

And then in answer to a following cross question he said:

10325
> "This junction between the spark gap and the "circuit of the dummy antenna was not made at "potential node, or at least with that object in "view, but the adjustments were always made for "best operation of the set."

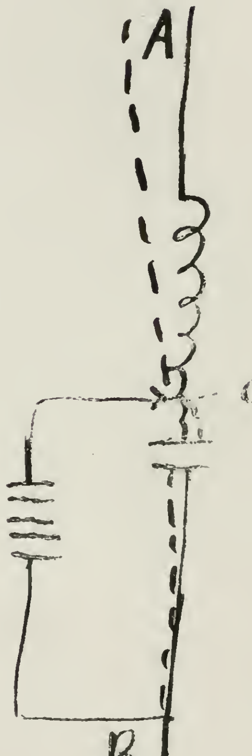
Dr. Chaffee has stated on typed page 31 that he has read the testimony given in this case by Mr. Simpson. Mr. Simpson pointed out very clearly indeed the reasons why it was essential to connect the trigger to the antenna at a potential node. In view of this I am surprised at his omission to do so.

Even at the risk of repetition I would like to point
10326 out as briefly as possible the reason why this potential node connection is essential. An electrical circuit such as the antenna circuit of the Simpson mercury valve transmitter vibrates in a manner not unlike that of a stretched string—violin string, for example. Now, to continue this acoustical analogy, it is well known that whenever a violin player wishes to play a harmonic note he does this by lightly and momentarily touching the string at a nodal point.

I can make this clear by a sketch which I now produce and which I will call "GWP66 'nodel point connection' "

3443

Point Connectio



 10333

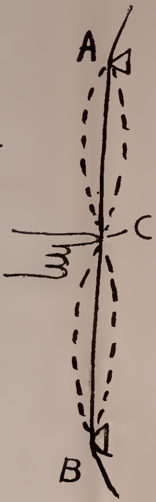
(reproduced opposite). In the lefthand figure of this sketch I have shown a stretched string AB with the violinist's finger C making contact with the string at a nodal point, and I have shown by dotted lines the manner in which the node is set up at the point C. In the right hand figure of my sketch I have shown in elementary form the antenna circuit of the Simpson mercury valve transmitter at AB, and at the point C the trigger lays its fingers, so to speak, on this nodal point, with the result, as I have shown by a dotted line, that a potential node is established at this point.

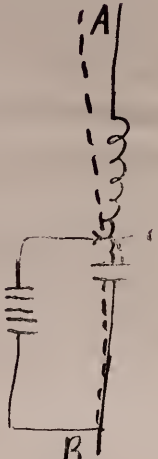
When this nodal point connection of the trigger to 10334
the antenna circuit AB of my sketch "GWP66" is properly made, there will be practically no potential across the points CB during the oscillation of the system AB. Prof. Morecroft has, I believe, criticized this matter of the nodal point on the ground that inasmuch as the system AB is radiating energy from its upper portion, that there can be no true node. Of course, to a slight degree; that is to say, whenever one part of the system is losing or gaining energy greater than another, a nodal point between these two portions of the system will be subject to slight potential variations, so that across the point CB instead of zero potential, there might be per- 10335
haps several hundred volts. However, to all practical intents and purposes, there is no difference of potential across the point CB, first, because the system AB has a comparatively low, or small decrement, and therefore is losing energy at a slow rate, and, secondly, because a slight potential fluctuation across the nodal point CB is immaterial because the spark gaps will not break down because of such slight potential fluctuations.

Because, as Dr. Chaffee has admitted, this nodal point connection was not made, I therefore consider that for this reason alone the Simpson mercury valve transmit-

G.W.P. 66

Nodal Point Connection





George E Brown

ter during the tests on July 3rd and 4th, was not in normal operating adjustment.

Mr. Farnsworth: I offer in evidence as Defendant's Exhibit the Pickard diagram "GWP66 'nodal Point connection' " made by the witness.

Mr. Betts: I object to that as immaterial.

It is now 11:00 o'clock A. M., and I desire to advise Mr. Farnsworth that I, together with Messrs. Weagant and Waterman, am leaving for Chicago at 5:30 this afternoon for Seattle, to resume the trial of this case, and because it is neces-
10337 sary that I should cross examine any witnesses produced by the defendant, it will be necessary for Mr. Farnsworth to produce any other witnesses which he proposes to call in New York, so that I may have an adequate opportunity to cross examine them before 5:00 p. m. today, the usual hour of adjournment.

I make this statement now in order that Counsel on the other side may be advised, since I understand that Mr. Pickard, the witness now on the stand, is to be in Seattle in this case, and his testimony can be resumed there.

Mr. Farnsworth: Defendant's plans for fur-
10338 ther testimony of the trial at Seattle have not yet been completed.

Last week in Boston Defendant's representatives attended the tests and testimony uniformly as late as six p. m., and on occasions as late as 8:30 p. m., and without protest attended the plaintiff's alleged tests on the 4th of July, a day not noticed.

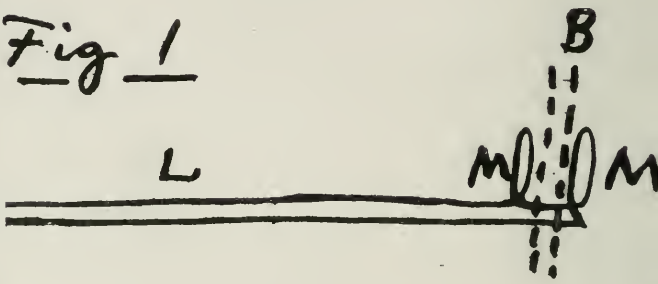
To-day we will endeavor to proceed in orderly fashion, extending all considerations to Plaintiff's Counsel, but this is the last day allowed us for testimony in New York, and we must proceed.

3447

Orig.

Tube Connection.

Fig 1



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Mr. Betts: I have advised Mr. Farnsworth thus early in the morning of his last day of testimony in order that he may, if he proposes so to do, now call Dr. Zenneck, or any of the other witnesses, whom he has given us notice he proposes to take the testimony of in New York, so that I may have an opportunity to adequately cross examine them before I leave at 5:00 p. m. to-day.

Q. State in detail the facts which constitute the reasons for your statement in this deposition that the Plaintiff's Massachusetts tests were invalidated among other 10346
things, by the arrangement of the leads from the radio telegraph transmitter to the Braun tube? A. As I have already stated, the current, or magnetic deflecting coils, of the Braun tube were not directly included in the trigger of the Simpson mercury valve transmitter, but were instead connected by means of long leads across a portion of the inductance in the trigger; that is to say, across a portion of one of the leads running from the spark gap to the condenser.

To make this clear I now produce a sketch, G. W. P. 67 entitled "Braun tube connection" (reproduced opposite). In Fig. 1 of this sketch I have shown the actual connections, the Braun tube B being shown in 10347
dotted lines passing between the deflecting coils MM, which are connected by long leads L to the trigger across the point X, which is a portion of the lead joining the spark gap T with the condenser C.

In Fig. 2 of my sketch I have shown what this connection is from an electrical standpoint. The portion of the condenser X in Fig. 1 is really (so far as rapid current changes are concerned) an inductance, such as the inductance I have shown at X in Fig. 2. This makes the connection of the leads L to the trigger a form of auto-

G.W.P.67 Orig.

Braun Tube Connection.

Fig 1

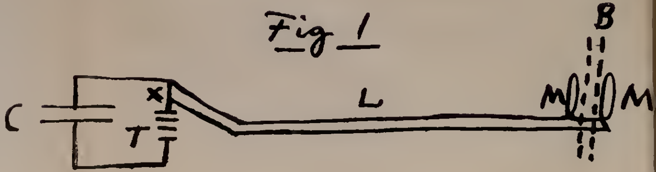


Fig 2

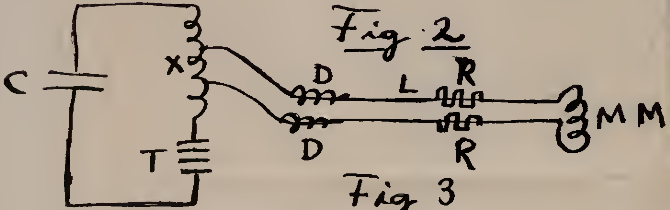
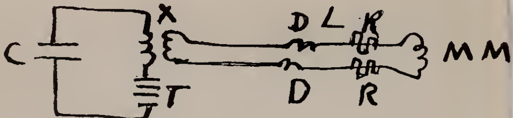


Fig 3

Geo E. Brown



transformer coupling, which is electrically an exact equivalent of the coupling which I have shown in Fig. 3 at the point X.

Assuming that a single one way or unidirectional pulse of current occurs in the circuit CXT of either Fig. 1, 2 or 3, an alternating current must exist in the circuit XLM, as shown in any of the figures, 1, 2 or 3. That is to say, the current flowing through the coils MM is not the same kind of current which exists in the circuit CXT, so that therefore the Braun tube cathode stream will not be deflected in accordance with the current flowing in the circuit CXT, but will be deflected in accord with the dif-
10349 ferent form of current flowing in the circuit XLMM.

If, during the tests on July 3rd and 4th at the Cruft's laboratory, there had been in the circuit CXT a current in the form of one way or single half oscillations, the Braun tube could not have shown this, but instead would have indicated a current of at least two half oscillations.

As I have shown in Fig. 2, the lead circuit L of Fig. 1 really possesses a material inductance, and this I have indicated in Figs. 2 and 3 at the points DD. Also, because of the fact that the leads were long and the frequency high, a material amount of resistance was included, and this also I have shown in the conventional
10350 symbol at the points RR in Figs. 2 and 3. If, which was not the case during these tests, the resistance of the leads L of Fig. 1 of my sketch had been made negligible, and also the resistance at the point X had been made very large, then a fairly correct representation of the electrical phenomena in the circuit CXT might have been reproduced from the screen of the Braun tube B. As this was not done, I consider all of Dr. Chaffee's photographs made with this connection to be inaccurate, and that they do not represent at all what is happening in the circuit CXT.

 10351

Further, owing to the fact that the leads L were quite long, and also rather close together, a very material electrostatic capacity existed in the circuit, so that oscillations might be set up in this circuit by impact from the circuit CXT, and these oscillations be recorded by the Braun tube.

In my opinion, the only fair way of making such a test would have been to directly include the coils MM of Fig. 1 of my sketch in the circuit of CT at the point X, so that all of the current in the circuit should pass through these coils.

Mr. Farnsworth: I offer in evidence the 10352
sketch made by Mr. Pickard in connection with the last answer, as G.W.P. 67 Braun tube connection.

Mr. Betts: Objected to as immaterial.

Q. Assuming, contrary to the facts as testified to by you, that the Plaintiff's Massachusetts tests were in fact valid and did in fact conclusively demonstrate the action of a Simpson mercury valve transmitter, on that assumption state whether or not said tests showed that the Simpson transmitter is or is not of the impulse or impact type?

10353
You may preface your answer by giving a brief definition of an impulse type transmitter as distinguished from any other type of transmitter.

Mr. Betts: Objected to on the ground that this question is directed to the question of infringement, and hence outside the scope of the order under which this testimony is being taken.

And I again protest against consuming time today in the examination of this witness, if it is the intention of the counsel for the Defendant to ex-

amine Dr. Zenneck or any of the other witnesses whom he has given us notice that he proposes to examine in New York, and I further advise Defendant's counsel now that I shall urge that the putting on of Mr. Pickard late yesterday afternoon and continuing his examination today is for the purpose of consuming time so as not to allow me adequate time to cross examine any other witnesses before my departure this afternoon for Seattle. As stated before, I am perfectly willing to agree to suspend Mr. Pickard's deposition in
10355 order to allow the Defendant to call any of the other witnesses.

A. An impulse type transmitter is one in which there is no reaction between the charging or supply circuit and the radiating or antenna circuit. It is a transmitter in which the energy from the supply or charging circuit is supplied to the antenna, and then cut off before there can be any retransfer of energy back from the antenna. I can explain this best by reference to the coupled pendulum records appearing on Defendant's Exhibits G. W. P. sketches 37, 38 and 39. (See Vol. 3.) In pendulum record G attached to G. W. P. 37 is shown graphically
10356 the condition existing in coupled oscillatory circuits, wherein the energy is transferred back and forth several times, this successive transfer and retransfer being shown by the beats produced. A transmitter operating in this way is not an impulse transmitter.

In record H attached to G. W. P. 38 is shown the transfer from the charging or supply circuit (shown in black on this sketch) to the radiating oscillating circuit (shown in red), before any reaction or any retransfer of energy takes place. In this record H two or two and one half complete oscillations exist in the supply circuit, and at the end of these the supply circuit goes out of existence by spark gap quenching, and the antenna or radiating circuit then continues in free vibration for a relatively very long time. This is an example of one of the types of impulse transmitter.

In record I attached to G. W. P. 39 is shown the action of an impulse transmitter operating by a single chunk or single half oscillation in the supply circuit. Here also there is no reaction or retransfer of energy between the antenna or radiating circuit and the supply circuit.

In Defendant's Exhibit G. W. P. 42 (See Vol. 3) I have shown a classification of transmitters, in which 10358
under the head of "impulse type" I have placed both the beat impulse type and the single shunt type.

Assuming now that the tests at the Cruft's laboratory were correctly conducted, and that the Simpson mercury valve transmitter was in normal operating condition, with the spot on the Braun tube screen correctly showing all the electrical current variations in the trigger, in my opinion these tests would then conclusively show that the Simpson mercury valve transmitter was of the impulse type, converting all of the energy existing in static form in the condenser into useful oscillatory form in the antenna before any reaction occurred between the antenna and the trigger. That is to say, it 10359
would be of the beat impulse type transmitter.

Plaintiff's witness Prof. Cross on typed pages 162 and 163 in interpreting the Braun tube photographs taken by Dr. Chaffee has unintentionally given a most misleading word picture of the conditions existing in the trigger and the antenna. He says:

> "This curve shows first a gradual building up of the antenna current and afterwards, at a later stage, it is dying away, the vibrations in the first part of the curve growing stronger and stronger, and in the latter weaker and weaker. The increas-

> ing portion of the envelope represents the gradual building up of the oscillatory current in the antenna, under the influence of electrical oscillations in the primary circuit. The diminishing portion, falling to zero, indicates the condition of gradual diminution of oscillatory current in the antenna as these decay, being no longer under the influence of oscillations in the primary."

I think anyone reading this portion of Prof. Cross's testimony would be led to suppose that the primary, or supply circuit, was in electrical activity for a relatively
10361 great part of the time that the antenna circuit was in operation. That, indeed, "the gradual building up of the antenna current" was in no way different from the "gradual diminution of the oscillatory currents in the antenna".

As a matter of fact, Dr. Chaffee's photographs and his testimony relating thereto, do not in any way support such a conclusion. Dr. Chaffee has several times stated that there were only two or two and a half oscillations in the trigger, and that there were in the antenna circuit a train of 50 to 100 oscillations. In fact, Dr. Chaffee's testimony as to this is entirely correctly represented by the record H attached to "GWP38" where in the sup-
10362 ply circuit some two complete oscillations existed, and in the antenna circuit a train of some seventy oscillations resulted. There is no gradual building up of antenna current. Instead, there is a most abrupt rise in antenna current from zero to its maximum value, this maximum value occurring at the instance of the first beat in the supply circuit, and thereafter a very gradual diminution of current in the antenna circuit, and no current activity whatsoever in the supply circuit.

In order to avoid any misinterpretation as to the meaning of the term "impulse transmitter", I would like to here state that it is not in any way dependent on any particular number of oscillations in the supply circuit. An impulse transmitter remains an impulse transmitter whether there be half oscillation in the supply circuit, two and a half complete oscillations or eight or ten complete oscillations. The sole criterion is whether or no there is any reaction or retransfer of energy between the supply and the antenna circuit.

In connection with Plaintiff's tests of July 3rd and
4th on the Simpson mercury valve transmitter, it must be
borne in mind that these tests, and the testimony relating 10364
thereto, have not in any way contradicted either the fact that the Simpson mercury valve transmitter is an impulse type of transmitter, or that it is a single circuit transmitter in which the energy is initially placed in and on the single oscillatory circuit at the time of the charging of the condenser, and remains in and on this circuit until it is finally radiated away. I have very carefully read over the testimony of Dr. Chaffee and the other witnesses of the plaintiff, and I do not find anywhere any statement that the transmitter is other than a single circuit transmitter; that is to say, a transmitter in which the energy
is placed in and on a single circuit, and there converted 10365
into oscillations and as these oscillations persist in this single circuit, radiated therefrom.

Mr. Betts: The whole of the answer is objected to as argumentative and further objection is made to that portion of the answer which purports to quote a phrase from Prof. Cross's testimony, which has been separated from its context.

Q. In respect of a Braun tube having deflection plates outside the tube, the plaintiff's Massachusetts witnesses, including particularly Prof. Cross, have criticised Defendant's use of such a tube with outside plates in connection with the Simpson mercury valve transmitter. Will you please state the facts in this respect? A. Plaintiff's witnesses have criticised the tests made at Washington University at Seattle on two grounds; first, that the Braun tube employed had outside electrostatic deflection plates, and secondly, that the connection was made across the spark gap.

I am quite familiar with the Braun tube oscillagraphic work, having in past years witnessed many tests made
10367 with the Braun tube by Prof. G. W. Pierce, and others. I am also quite familiar with the literature of this subject. Prof. J. A. Fleming, technical adviser of the Marconi Company, in his book entitled "The Principles of Electric Wave Telegraphy and Telephony", 1910 Edition, at pages 30 and 31, discusses the use of the Braun tube for the delineation of oscillatory current phenomena. At page 31, Fig. 28 is shown a "method of employing the Braun cathode ray tube with electrostatic deflection plates for delineating condenser discharge currents."

Now, the delineation of condenser discharge curves is precisely what was done at the Washington University
10368 at Seattle and also attempted at the Cruft Laboratory on July 3rd and 4th.

An examination of Fig. 28 of the Fleming book would show two things; first, that outside deflection plates PP are employed to deflect the cathode ray up and down, and secondly, that these plates are connected directly across a spark gap shown just above the condenser K in the oscillatory circuit KSL.

Prof. Fleming says as to this arrangement on page 30:

> "The Braun tube T has a cathode terminal led to the negatives of a Voss machine driven by a

 10369

> small electric motor. Two brass plates PP (See Fig. 28) are placed on either side of the tube just beyond the diaphragm in it, and these are connected with spark balls of the oscillatory circuit containing a condenser K, and an inductance L."

He also says that this description and figure was taken from the words of Messrs. Varley and Murdoch published in the Philosophical Magazine in 1902, Series 6, Volume 3, page 500, and also "The Electrician, 1905, Volume 55, page 335."

So far as the objective representation of the currents
in the trigger circuit of the Simpson mercury valve transmitter is concerned, I should therefore consider that there would be no objection to using outside deflection plates and to connecting these across the spark gap, precisely in the manner set forth by Prof. Fleming. I do not dispute the statements of plaintiff's witnesses as to the shift of zero which may occur by this mode of connection, but I do not consider that this zero shift will in any way mask the representation of the oscillatory or non-oscillatory character of the circuit to which they are connected. 10370

It is true that to a very slow variation of potential
across the outside electrostatic deflection plates that the accumulation of charge inside the tube might more or less completely mask the representation of an alternating current, say, of sixty cycles. But for the far more rapid variations of potential occurring in the supply circuit of an impulse transmitter, I do not think that the objective representation on the screen of the Braun tube would be seriously impaired. 10371

> Mr. Farnsworth: I offer in evidence pages 30 and 31 referred to by the witness in the last answer of the 1910 edition of the Fleming work en-

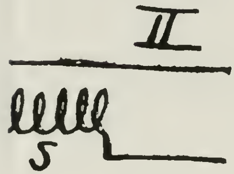
titled "The Principles of Electric Wave Telegraphy and Telephony" as Defendant's Exhibit "Fleming Book, Extracts on the use of Braun tube for delineating condenser discharge curves."

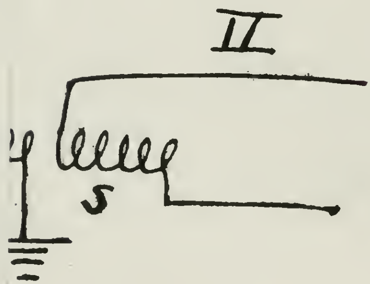
Q. Referring to Plaintiff's Seattle tests of Defendant's standard receiver, in which said receiver was modified by the Plaintiff by the insertion of a condenser in shunt to the secondary coil; assuming that as a result of such modification there was any improvement in operation or increase of strength of signals, please state
10373 to what such result was due?

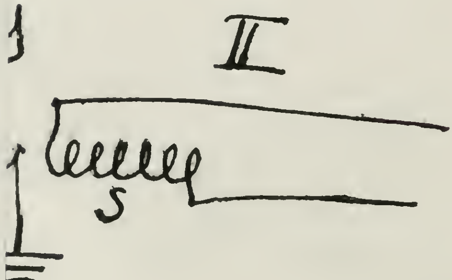
Mr. Betts: I object to this question and any answer that may be given on the ground that it is not within the purview of the order under which this testimony is to be taken, since on its face the present question does not in any way relate to the Simpson mercury valve transmitter, nor to the tests at the Cruft High Tension laboratory on July 3rd and 4th. Secondly, on the ground that no permission has been obtained from the Court to take any testimony on the part of the Defendant in surrebuttal in relation to the Defendant's receiver, except that noted on the record. Plaintiff's
10374 counsel is of course helpless at this time to prevent the witness from answering this question, but gives notice that he will move to strike out the question and any answer that may be given.

I again protest against consuming time today in the examination of this witness, if it is the intention of counsel for the other side to call Dr. Zenneck or any of the other witnesses of whom he has given notice on the record in regard to the Massachusetts tests. I have expressed a willingness to suspend the examination of this witness in order

ing Change Resulting From
th Change.







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that the Defendant may call others, if so advised, prior to my departure for Seattle today.

A. In the tests performed by the Plaintiff on the Kilbourne & Clark standard receiver, modified by the addition of a condenser in shunt to the secondary, it was observed that while up to a wave length of about six hundred meters the receiver operated much better when no condenser was used, for wave lengths over six hundred meters an increase of efficiency could be obtained by the addition of the condenser. The reason why this was so may be readily understood from a considera- 10382
tion of the sketch which I now produce, G. W. P. 68, entitled, "Coupling change resulting from wave length change" (reproduced opposite). In Fig. 1 of this sketch I have shown, somewhat diagrammatically, the short primary winding employed for wave lengths including and below six hundred meters. The secondary winding S cannot be coupled with the primary winding P tighter than a certain maximum determined by the construction of the receiver; that is to say, its nearest position to the primary P is somewhat as I have shown it in Fig. 1. When a longer wave length is received, a greater length or number of turns of wire in the primary winding P is employed, as I have 10383
shown in Fig. 2. This results in bringing the body of the coil P in effect further away from the secondary S, and so decreasing its coupling therewith.

In Fig. 3 I have shown the conditions in the Kilbourne & Clark standard receiver when a very long wave length is received. Here the primary winding is divided in two coils, P1 and P2, and only the coil P2 is in inductive relation with the secondary S. This of course still further weakens the coupling.

Bear in mind that this receiver was constructed for

GWP 68

Coupling Change Resulting From Wave-Length Change.

Fig. 1.

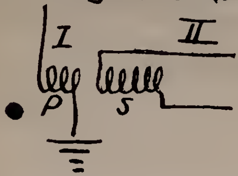


Fig. 2.

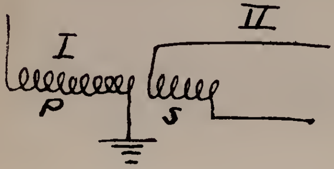
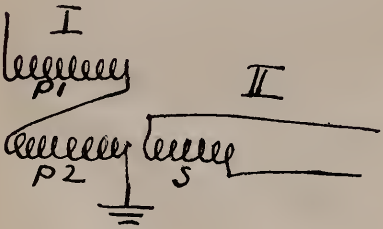


Fig. 3.



George E Brown

10384

commercial service, wherein the wave lengths employed were six hundred meters or less, the maximum coupling was naturally chosen as that giving the optimum conditions for such a wave length range. While the primary winding and circuit was so arranged that much longer wave lengths could be received, yet no particular stress was apparently laid by the designer on obtaining the optimum coupling for these longer wave lengths.

When the Plaintiff added a condenser in shunt to the secondary winding S, the effect was simply that of tightening the coupling. This effect is well recognized in the
10385 art, and I find for example in the "Manual of Wireless Telegraphy for the Use of Naval Electricians," written by Lieutenant Commander S. S. Robison, U. S. Navy, and Dr. L. W. Austin, published in 1909, this very matter is discussed on Pages 97 and 98. On Page 97, Fig. 49, is shown a receiving circuit identical with that of the Kilbourne & Clark standard receiver, having in the primary or antenna circuit a variable inductance L and a variable condenser C, the inductance L being inductively linked to the secondary circuit KD, containing a secondary winding, a stopping condenser K, and a detector D, all in simple series arrangement. The text states on Page 97, referring to the receiver of this figure, "In
10386 this case the detector circuit is untuned."

Then, on Page 98, Fig. 49a, is shown the same circuit as that of Fig. 49, with the addition of a variable condenser C2 placed across the secondary winding L2, precisely as was done by Plaintiff in the tests of the Kilbourne & Clark standard receiver at Seattle. Regarding the effect of this added condenser, the Manual states, Page 98, "If we place a variable condenser C across the "inductance of the detector circuit as shown in Fig. "49a, we may bring the detector circuit into resonance "with the incoming waves. Now the effect of tuning the

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"circuit is simply to nullify the reactance of the inductance L^2 by means of the condenser, and the position "of most favorable coupling can then be obtained with "a greater distance between the coils L^1 and L^2."

In other words, the effect of adding this condenser is to tighten the coupling between the primary and secondary coils. Inasmuch as coupling between any two circuits is directly proportional to the mutual inductance and inversely proportional to the square root of the sum of the separate inductances in the circuits, obviously then if we nullify the reactance to one of these inductances,
as by the use of a suitable condenser, we increase or 10388
tighten the coupling; and inasmuch as for wave lengths over six hundred meters the coupling in the Kilbourne & Clark standard receiver was not quite sufficient for the best results, the addition of this condenser sufficiently tightened the coupling to give a greater amount of energy in the detector circuit and a louder signal.

This action in no way depends upon the utilization of the principle of resonance between the two circuits, the antenna circuit and the detector circuit, or any gradual building up or accumulation by the aid of resonance of energy in the detector circuit, but instead depends simply, as the Navy Manual states, upon obtaining a position of most favorable coupling. 10389

If the principle of resonance or gradual building up of energy in the secondary circuit had been taken advantage of, why then such a form of receiving circuit as shown in Fig. 49a of the Navy Manual would naturally have a very marked increase in selectivity, as compared with the untuned detector circuit form shown in Fig. 49. But the Manual states at the bottom of Page 98,

> "There is an apparent advantage in selectivity "in the case of the use of the tuned secondary

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"receiving circuit, inasmuch as slight changes in "the secondary condenser are often sufficient to "tune out the incoming signals. But the higher "degree of selectivity is only apparent, as what "is really done is to throw the secondary out of "tune with the antenna, which has nothing to do "with the true selectivity regarding the signals "received."

My own experience with such circuits places me in entire agreement with the Navy Manual statement, as to there being no gain in selectivity when a secondary condenser is used.

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Mr. Betts: In addition to the objections heretofore made to the present question and answer, and notice of motion to strike out, I further object to the question on the following grounds:

First, that it is argumentative.

Second, that it is incompetent, in that it presupposes an attitude of mind in the designer of the standard Kilbourne & Clark receiver, of which the present witness has no knowledge.

Third, that the witness has merely quoted from the Navy Manual of 1909, which Navy Manual was superseded by later manuals already in evidence
10392 in this case, but a fragmentary portion of the context.

Mr. Farnsworth: I offer in evidence as Defendant's Exhibits the entire pages 97 and 98 of the 1909 Navy Manual referred to in the last answer; also the drawing G. W. P. 68, entitled "Coupling change resulting from wave length change," produced by the witness in connection with the last answer.

Mr. Betts: As to the offer of the pages from the Navy Manual of 1909, objection is made on

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the ground that said Manual has been superseded by later editions of the Navy Manual, which have already been offered in evidence in their entirety in this case.

As to the sketch of the witness, G. W. P. 68, this is objected to as immaterial and incompetent, on the ground that it presupposes the intent of the designer of the standard Kilbourne & Clark receiver, of which the present witness has no knowledge.

Q. Referring to Defendant's Seattle tests with forms 10394
of the Defendant's Thomson impulse transmitter which were forms resulting from Plaintiff's modification consisting of the use of various metal loops, especially manufactured by the plaintiff, and inserted in the spark circuit of that transmitter, state whether or not those tests showed any use even under the said altered and abnormal conditions of that transmitter of the principle of electrical resonance between the spark circuit and the antenna circuit, and state your reasons.

Mr. Betts: Objection is made, and protest made, to this question on the ground that it was
outside of the intent of the stipulation and order 10395
under which this testimony is taken, which was to be limited to testimony in regard to the tests conducted at the Cruft High Tension Laboratory on the Simpson mercury valve transmitter; that this question is in the nature of sur-rebuttal testimony for which no order of the Court had been obtained by the defendant. The plaintiff is helpless, of course, being more than three thousand miles away from Seattle to prevent the witness from answering the question. He gives notice he

will move to strike out the question and any answer that may be given, and furthermore again he reiterates the statement in regard to other witnesses.

A. In the tests made by plaintiff using their specially manufactured wire loops, one thing was very strikingly brought out. Regardless of the form and dimensions of the loop used and in consequence equally regardless of the difference in tune or time period between the two circuits of the transmitters, the radiated wave remained
10397 a single pure wave, having always the same wave length which was the free wave length of the antenna circuit. If there had been any utilization of resonance between the two circuits; that is to say, any cumulative building up of energy in the secondary circuit by a gradual transfer from the primary or supply circuit, a complex wave would have resulted in the secondary circuit in which two or more humps would have been found, and in general every time a new and different loop was placed in the primary circuit a different wave length, or wave lengths would have appeared in the secondary or antenna circuit. But the only effect of any or all of these various loops manufactured and inserted in the supply circuit by the
10398 plaintiff was to vary the amount of current in the antenna circuit and not its frequency.

(Recess.)

(After Recess.)

Greenleaf Whittier Pickard resumes the stand.

A. to Q. (continued) At the request of the Plaintiff, the assessors noted the antenna current and wave length

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in the antenna circuit, at each of the tests made with the various wire loops. These observations of the assessors very clearly show that the wave length remained very closely at six hundred meters, and that it was a single pure wave in all of the tests; whereas the apparent wave length of the charging circuit varied over wide limits. If there had been any resonance tuning between the charging circuit and the antenna, this would not have been the case. A complex wave would have resulted in each instance, and the wave meter would have shown two or more humps or wave lengths.

Mr. Betts: The last answer of the witness is objected to as argumentative, in addition to the reasons heretofore entered of record. 10400

The cross examination is without waiver of objections.

Cross examination by Mr. Betts:

XQ. You heard or have read the testimony of Messrs. Chaffee, Coffin, Morecroft and Cross taken last week at Cambridge, Massachusetts, at the Cruft High Tension laboratory, Harvard University? A. Yes.

XQ. And prior to giving your present deposition you examined the negatives produced by Dr. Chaffee? A. I 10401
examined all of the negatives produced by Dr. Chaffee, save those negatives produced by him at the request of Mr. Farnsworth.

XQ. You examined all of the negatives which were taken during the tests of July 3rd and 4th? A. Yes.

XQ. What do you mean by "resonance tuning"? A. Why, I mean tuning between resonance circuits, that is to say, between oscillatory circuits, the adjusting of one such oscillatory circuit to have the same period as a second oscillatory circuit.

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XQ. And while you were at the Cruft High Tension laboratory, Harvard University, on July 3rd and 4th, you witnessed all of the tests conducted by Dr. Chaffee on the Simpson mercury valve transmitter? A. Yes.

XQ. What wave length do you assume was received, in Fig. 3 of your sketch G. W. P. 68? A. Some wave length materially longer than six hundred meters.

XQ. Any particular wave length longer than six hundred meters? A. No, except a longer wave length than that shown in Fig. 2 of my sketch G. W. P. 68, which is also a wave length longer than six hundred meters.

10403 XQ. How much longer than six hundred meters did you assume the wave received was, as depicted by you in Fig. 3 of your sketch 68? A. Of the order of some three or four times longer than six hundred meters; that is to say, a wave length of two thousand meters or over. I am not entirely sure just where in the circuit as represented in Fig. 3 of my sketch G. W. P. 68 the coil P1 is cut into the circuit; that would depend somewhat upon the constants of the antenna, but according to my recollection, for wave lengths over about two thousand meters this coil P1 had to be employed on any ordinary antenna.

XQ. As I understand your present definition of an impulse transmitter, such a transmitter may comprise
10404 a plurality of complete or half oscillations in a primary or spark gap circuit, provided there is no retransfer of energy from the antenna circuit to the spark gap circuit; is that correct? A. That is entirely correct.

XQ. Assuming that Dr. Chaffee's tests were properly conducted on the Simpson mercury valve transmitter, and that that transmitter was properly connected, and in normal operative condition, then as I understand you the operation of the Simpson mercury valve transmitter would be such as is shown in record H of your sketch G. W. P. 38? A. Yes, that is correct.

 10405

XQ. During your witnessing of Dr. Chaffee's tests on July 3rd and 4th at the Cruft High Tension laboratory, did you examine the spark gaps during the tests to ascertain if they were overheating? If so, on what particular test or tests did you make that examination? A. I only examined the spark gaps during the tests by wave meter and stroboscope observations. I did not open any of the gaps during the test, nor, so far as I recall, did I determine by touching the gaps whether they were overheating. The construction of the gap is such as to make this difficult.

XQ. During what particular test or tests did you ex- 10406
amine the spark gaps by the method you stated in your last answer? A. These examinations were made at intervals all during the tests of July 3rd and 4th. I think I listened at the wave meter and observed the stroboscope during some twenty or thirty tests, and at times when some ten or twelve photographs were being taken by Dr. Chaffee. I made no record of the exact number of times that I made these observations, so that I cannot give the exact number or the exact times when they occurred with reference to any of the photographs taken.

XQ. Then you cannot now state any particular test or tests during which you examined the spark gaps by the
method you have stated in your next to last cross an- 10407
swer? A. I can only say that those observations were taken at intervals, and at times that I considered as representative of the tests. I cannot definitely connect the times of these observations with any of the numbered photographs taken by Dr. Chaffee.

XQ. Are operator's adjustments provided on the Simpson mercury valve transmitter, whereby it can be made to operate with partial discharges? A. No specific operator's adjustment is provided on the Simpson mercury valve transmitter for such operation. An operator could, by sufficiently raising the voltage of the generator, as by manipulation of the field rheostat, obtain such a voltage as might produce partial discharges.

XQ. Do the spark gaps on the Simpson mercury valve transmitter which was tested at the Cruft High Tension laboratory at Harvard University now, after all of the tests of July 4th are concluded, show any evidences of overheating such as you said would result from partial discharges? A. I have not seen these gaps opened since some time in the afternoon of July 4th. At the time of my last observation of the opened spark gaps on July 4th, I did notice most distinct signs of such overheating,
10409 consisting in a large gauged out or crater-like depression on one of the plates.

XQ. Were all of the spark gaps on this transmitter oxidized to the same extent at the completion of the tests? A. When the spark gaps were opened the second time, on July 4th, after some hour or so of testing, I think the surfaces were oxidized to approximately the same extent on all the gaps. I do not now recall any marked difference between the appearance of the gaps.

Q. As a matter of fact you know that the gaskets were not damaged by the plaintiff's representatives on some of the gaps? A. All that I can now recall is that in
10410 one of the gaps the gasket was very badly broken. The others I did not examine carefully but I recall that the gaskets were not damaged to the same extent, or perhaps not to an extent noticeable in my casual examination.

Do you think that Dr. Chaffee was unfair in his tests because he adjusted the Simpson mercury valve transmitter to get the best operation and the highest efficiency?

Mr. Farnsworth: Objected to as manifestly unfair.

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A. I do not know that Dr. Chaffee did either of those two things. That is to say, adjusted the transmitter to the best operation or the maximum efficiency. I am in no way questioning Dr. Chaffee's good faith in the matter. I think he did the best he could with the transmitter according to his theory of how it should be operated.

XQ. Have you any reason to believe that Dr. Chaffee, when he adjusted the Simpson mercury valve transmitter to maximum efficiency did not as a matter of fact connect the spark circuit to the nodal point? A. I have only Dr. Chaffee's own testimony as to that in which he states
that he made no attempt to adjust, or fix, the trigger to 10412
the nodal point.

XQ. You do recall, do you not, that Dr. Chaffee testified that he adjusted the Simpson mercury valve transmitter to maximum efficiency? A. To what he considered maximum efficiency, yes. I may say that I have no means of determining whether or no the set was operating at best efficiency because of the lack of the customary indicating instruments in the various circuits; that is to say, I had no means of knowing what the power input was or, indeed, what the antenna current might be.

XQ. You could have determined those factors if you
had applied measuring instruments? A. I could have 10413
very readily and quickly determined all those things. As a matter of ordinary practice I should have done that in any test made under my supervision.

(Sgnd) G. W. Pickard.

Testimony Closed.

Mr. Betts: I inquire whether there are any further witnesses to be called today?

Mr. Farnsworth: I have no intention of calling any more, and as soon as Mr. Pickard has read

10414 through the typed copy of his deposition the session will be adjourned *sine die.*

Mr. Betts: In view of the statement of Mr. Farnsworth Counsel for the Plaintiff now will leave the room.

Mr. Farnsworth: If Mr. Pickard finds any necessity for adding anything to his deposition after having read it, he will do so, and then the session will be closed. This remark I make in the presence of Mr. Betts. The same remark also applies to Prof. Stone and Mr. Simon, who are now correcting their depositions.

10415 Mr. Betts: I deny the right of defendant's counsel to have any of these witnesses add matters to their depositions which have already been concluded, by way of new matter. The depositions were taken under order of the Court stenographically just as if taken in open Court and, of course, I have no objection to the witnesses correcting their depositions already given in any immaterial way before they sign it, but it is obviously unfair to Plaintiff's Counsel to ask him to wait around for two or three hours until the deposition of Mr. Pickard has been reduced to typewriting so that

10416 he may read it.

Mr. Farnsworth: Pursuant to defendant's rights I shall keep this session open this day until the depositions of the witnesses have been completed, it being now 2:45 P. M. and Mr. Betts being about to leave the room.

Mr. Betts: I am ready to attend on any depositions between now and five o'clock. I ask Counsel to proceed if he has any further witnesses.

Mr. Farnsworth: We are now proceeding with

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all three witnesses correcting their depositions in typewriting.

Mr. Betts: In view of the evident desire of the Defendant's Counsel to embarrass Plaintiff's Counsel, Plaintiff's counsel will remain, as he considers Defendant's Counsel's attitude quite unprofessional.

I insist upon Defendant's Counsel also remaining in the examination room, but it is noted that Mr. Farnsworth leaves the examination room.

Mr. Farnsworth: I am back. 10418

Mr. Betts: He again leaves the room.

Adjourned *sine die.*

Mr. Betts: Mr. Betts notes that the adjournment is now taken at 4:35 after he has been kept waiting here since 2:45 P. M.

Mr. Farnsworth: The adjournment having been taken immediately upon the completion of Mr. Pickard's deposition and his corrections thereof and signing of the same and not later.

DEFENDANT'S SURREBUTTAL EVIDENCE. 10419

Benjamin Wolff, recalled as a witness on behalf of Defendant, testified as follows:

Q. (Mr. Skeel) Mr. Wolff, you are the radio inspector of this district, are you? A. I am.

Q. Please state what territory this radio district comprises? A. It comprises the states of Washington, Oregon, Idaho, Montana, Wyoming and Alaska.

Q. And you are familiar with the radio apparatus of the defendant Kilbourne & Clark Manufacturing Company, and have examined and tested it in the course of your official duties? A. I have, and I am familiar with the apparatus.

Q. I will ask you to state to the court what record is made by you as radio inspector in the course of your official duties of the primary inductance or loops in the spark circuit of the Kilbourne & Clark Thompson impulse transmitter?

> Mr. Hughes: How is it material what records are made, unless it be for the purpose of refreshing his memory?

10421 Mr. Skeel: The purpose of the question is first to find out what record he makes, and next I propose to offer some records.

A. When a radio apparatus is installed on board and it is found to be adjusted in compliance with the radio laws with regard to wave-lengths, decrement, etc.; when all this is done, the inspector takes physical measurements of the different parts of the apparatus, such as the inductance coils, the condensers, etc., and makes or plots a little drawing of the shape, diameter and number of turns of these coils, and this is a part of the record called the radio apparatus adjustment record. That is
10422 posted on the wall in the radio room.

Q. And is a copy kept by the radio office? A. A copy is kept by the radio office. This chart shows the number of turns in the secondary, the diameter, the length, the distance between the turns, the condenser, and the number of units in the condenser, and how they are grouped, the number of turns in the loading coil, the radiated current and the logarithmic decrement of the different wave-lengths.

Q. Now, with particular reference to the loop in the spark circuit of the transmitter, sometimes called the primary inductance, do I understand you to say that the record accurately describes that and gives the measurements? A. The record accurately describes the diameter and the number of turns, and practically all the physical measurements of that.

Q. Have you in your hand one of the adjustment cards of one of the vessels using a Kilbourne & Clarke transmitter? A. I have.

Q. What steamship does that apply to? A. This particular one applies to the Alameda.

Q. What is the showing on that record as to the pri- 10424
mary inductance? A. The primary inductance in this case, as shown by the chart, consisted of one loop, a single turn of inductance, eight inches in diameter, and one turn.

Q. Now, how often are the steamboats inspected with reference to the radio apparatus? A. All the boats are inspected before they sail each trip.

Q. And if any additional loops or any substitute primary inductance were found by the radio inspector what effect would that have upon the license of that radio system? A. If there would be any change at all from what is shown on the chart or as adjusted when the 10425
set was inspected for license and passed upon and licensed, any changes whatever would nullify the license.

Q. Is that a matter of regulation, or is that provided by the Radio Act? A. That is provided by the Radio Act.

Q. Have you ever known in the course of your official duties or otherwise any Kilbourne & Clark transmitters to have any added or substitute loops or substitute primary inductance? A. I have never seen but one loop provided for any one set, any one apparatus.

Q. I will show you plaintiff's exhibit No. 44, which comprises a number of additional loops or substitute loops furnished by plaintiff in some of the tests in this case, and ask you if your last answer referred to additional or substitute loops such as I hold in my hand? A. it did.

Q. Now, will you please state to the court also, Mr. Wolff, what record is made of the receiver. A. When an apparatus is installed on board a ship or station the owners or operators apply for license in the usual way, and fill out a form provided by the Government, called Form 761. This form calls for a description of the ap-
10427 paratus, the antenna, the transmitter and receiver. In the case of the transmitter, the antenna current and the logarithmic decrement for the different wave-lengths, the power supply to the transmitter, the type of spark gap, and the measuring instruments. Also in the receiver information is required as to the type, whether it is inductively coupled to the receiver, whether the secondary circuit is tuned or untuned, and whether or not the positions for 300 meters and 600 meters are plainly marked on the apparatus, and if the receiver is disconnected from the antenna when sending by a hand switch or automatic break. When this form is filled out it is passed upon by the radio inspector from the data
10428 obtained on his inspection, and forwarded to the Secretary of Commerce, the Department of Commerce, rather, with a report and recommendation as to whether a license shall be issued or not. Now, the license is granted upon that showing, is it, Mr. Wolff? A. The license is granted upon this application and the data furnished as shown by my inspectors.

Q. And will you state whether or not it is permissible for an owner or an operator or any one to change any feature of the appartus, either loops on the transmit-

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ter, or to insert additional or substitute condensers on the receiver? A. It is not permissible.

Mr. Hughes: One moment. You mean it is contrary to law to do it?

Mr. Skeel: Yes, whether it is permissible.

Mr. Hughes: The law would speak as to that, I suppose.

Mr. Skeel: Well, that is perfectly true. It is a matter of law.

Q. Now, Mr. Wolff, I will ask as a practical radio man 10430
whether or not there is any such thing as detector tuning for a receiving instrument?

Mr. Betts: I object to that as not proper surrebuttal, if the court please.

The Court: Oh, I think I will let him answer.

A. My experience, covering a period of over ten years, has convinced me that any tuning by the use of detector adjustments is not possible, or if it is possible, to no noticeable degree, no measurable degree.

Q. Have you in convenient form the radio laws of the United States and the Regulations of the Department of Commerce with reference thereto? A. I have. 10431

Q. I wanted to know if you have it in convenient form? A. Yes.

Mr. Skeel: I offer in evidence the radio apparatus adjustment record referred to by the witness for the Admiral Evans as defendant's exhibit No. 57.

I offer in evidence the radio apparatus adjustment record of the Department of Commerce for the steamship Alameda as defendant's exhibit No. 58.

I offer in evidence as referred to by the witness the applicant's description of apparatus in connection with the steamship Alameda as defendant's exhibit No. 59.

And merely for the convenience of the court I offer in evidence the radio laws and regulations of the United States, and also the regulations of the Department of Commerce with reference thereto.

Mr. Betts: As to the first three exhibits I should like to examine them to see whether I have
10433 any objections to make to the offer. As to the fourth I have no objection.

The Court: That may be filed. It is simply a matter of convenience. You may examine the others and register your objections.

(Pamphlet containing radio laws and regulations of the United States and also the regulations of the Department of Commerce received in evidence and marked "Defendant's Exhibit No. 60".)

Mr. Skeel: You may cross-examine.

CROSS EXAMINATION.

10434 Q. (Mr. Hughes) Mr. Wolff, the certificate, or what do you call this——

Mr. Skeel: It is on the top, "Radio apparatus adjustment record."

Q. The radio apparatus adjustment record which has been offered in evidence as defendant's exhibit No. 58, of the steamship Alameda, is dated July 25, 1915. Have you made any inspections of the defendant's radio apparatus on the Alameda since that time? A. I have made probably a dozen.

Q. Have you got a record of them? A. I have.

Q. How long would it take you to make out your report of the subsequent ones? A. How long did it take?

Q. To make out a copy like this exhibit. A. These copies are made only at the time the vessel's apparatus is inspected for license, and not on every ordinary inspection, just before they leave the port. Those are made once a year.

Q. Those are made one a year? A. Once a year.

Q. You make the same record in your own records each time you examine them? A. We make a record of the inspection of the apparatus and post the radio ap- 10436
paratus adjustment record card on the wall, and refer to that at the time of the inspection, at the time of subsequent inspections.

Q. Well, if there was any change in the radio apparatus what do you do in that event? A. That would nullify the license of the vessel, that is, the vessel would be operating then without a license.

Q. I may be mistaken, but I inferred from the testimony given in this case that some change or repair was made in respect to the Alameda since July 25, 1915. Did you examine it after that time? A. I have examined it. I have made about a dozen inspections, probably more,
of the Alameda, since July 25, 1915, and have checked 10437
up the physical adjustments of the apparatus as shown by the radio apparatus adjustment record card which is posted on the wall of the radio office on board the vessel.

Q. Have you the record of those subsequent inspections here? A. I have.

Q. May I see it? A. I have not got them with me. I have them in the building. I can get them.

Q. I would like to have you do so. Perhaps your examination can be resumed after you get them, if you will. Get them for the Alameda and the Admiral Evans, both

10438

of which are covered by the adjustment records introduced in evidence, and bring with you also the records of all other ships carrying Kilbourne & Clark apparatus. A. All others?

Q. Of the Thompson type. A. Did I understand you to say all others?

Q. Yes, the same record will contain all of them, wouldn't it, the same book? A. No.

Q. Bring those books that would contain the other inspections, a record of the inspections of the Kilbourne & Clark apparatus of the Thompson type? A. I am
10439 afraid I will not be able to do that inside of—it would take me probably a day to do that.

Q. They are here, are they not? A. I would have to search all through my files to get those.

Q. You will be here next week when we resume, will you not? A. Yes.

Q. Then I would rather defer the examination to that time.

Mr. Skeel: I want to ask one more question.

Q. As I understand you now, these adjustment records are made at the time the license is issued, is that correct? A. At the time the vessel is inspected for li-
10440 cense.

Q. And then one card is posted in the room, the radio room on board the vessel? A. That is correct.

Q. And the other record is taken to the radio office, is that correct? A. As a matter of record.

Q. Then prior to every sailing the vessel is again inspected and compared with the adjustment card? A. That is correct.

Q. And if there was any change a new license would be required? A. That is correct.

Q. And at the time that this adjustment card was taken and at the time any inspection was made subsequent to this adjustment card, under this card, there was only one primary loop found upon board the vessel? A. That is correct.

Q. What opportunity for inspection do you have? A. My duties require that I go on board and inspect the apparatus before each sailing, before they leave port, that is all.

Mr. Hughes: I will reserve my cross examination.

Mr. Skeel: If the court please, in regard to the request made upon Mr. Wolff, I would like to ask, 10442 in order to save time, if it would not be possible for counsel to inspect those records. I say this without asking Mr. Wolff.

Mr. Hughes: Probably I may be able to in his office, and in that way save time.

(Witness excused.)

Adam Lipke, produced as a witness on behalf of defendant, being first duly sworn, testified as follows:

Q. (Mr. Skeel) State your name. A. Adam Lipke. 10443

Q. Mr. Lipke, what is your occupation? A. I am a radio operator.

Q. And by whom are you employed? A. I am not employed by anybody at present, but I have been working for the Alaska Steamship Company.

Q. As operator on what boats? A. Why, I have been on the Jefferson—you mean all the boats I have been on?

Q. Yes, all the boats that you have been on that have been equipped with Kilbourne & Clark apparatus. A. I

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have been on the steamer Star, the Tatoosh, the Zaphora, and the Admiral Evans.

Q. Have you finished the answer? A. Yes, sir.

Q. Now, will you please state, Mr. Lipke, whether on the transmitter installed on any of those vessels you have ever used any additional or substitute loops or primary inductance in the transmitter? A. No, sir, I never have.

Mr. Hughes: Were all these vessels installed with the Thompson type?

10445 A. Yes, sir.

Q. What instructions have you, as a radio operator, Mr. Lipke, with reference to making any changes in the structure of your radio apparatus? A. I have instructions to leave a set exactly in the same condition as we find it, and under no circumstances except in case of distress to change it.

Q. Now, will you please state whether in any of the receivers of the Kilbourne & Clark Manufacturing Company installed on any of these boats there has ever been any substitute condenser or variable condenser placed across the secondary or detector circuit? A. No, sir, there never has.

10446 Q. Please state whether or not in your practice as a radio operator you have received messages over long wave-lengths, and to what extent, and over what distance? A. I have received messages from the Marconi station at Ketchikan, a distance of about 250 miles, at approximately 3,000 meters, and also I have heard the station at—while lying at Kodiak I have heard the station at—the place down in Washington near Astoria—North Head, Washington, I have heard him sending at about 1800 meters. We were about 2,000 miles off. We were at Kodiak at the time, lying alongside of the dock.

 10447

I also have heard the station at St. Paul Island and at Dutch Harbor sending on long meters, long waves.

Q. What kind of a detector have you used on the Kilbourne & Clark receivers? A. Crystal detector.

Q. Do you ever adjust the contact points? A. Yes.

Q. For what purpose and when? A. For the purpose of getting a selective spot on the detector, in order to make the signals come in good and strong, loud.

Q. And when you have adjusted it to a sensitive point how long do you leave it there? A. Why, we leave it there until it is knocked off by an extremely strong signal, or knocked out of adjustment in some way. Some- 10448
times it is left there for days at a time without being moved or adjusted again, readjusted.

Q. Do you make any variations or adjustments of the detector for the purpose of receiving signals on different wave-lengths? A. None whatsoever.

Q. And you say you leave it there for days at a time. During that time is it constantly in use? A. It is constantly in use.

Q. Receiving messages on different wave-lengths? A. Yes, sir.

Mr. Skeel: You may cross examine.

10449

Cross Examination.

Q. (Mr. Hughes) Mr. Lipke, you say you are not in any employment at this time? A. No, sir, I am not.

Q. How long has it been since you ceased work for the Alaska Steamship Company? A. Well, I have not worked for the Alaska Steamship Company for quite awhile, but for the Kilbourne & Clark apparatus that I was using, I have not used one of their apparatus for over six weeks. I have been ashore for over six weeks. I have not been working.

10450

Q. You have not been doing anything? A. Nothing, no, sir.

Q. Are you out of a job entirely? A. Yes, sir.

Q. Are you expecting to return to the service of any ships that use Kilbourne & Clark apparatus at any early date? A. Why, I have nothing in sight so far.

Q. No arrangement or understanding of that kind? A. No, sir, I have no understanding.

Q. What was the last boat you were on? A. The last boat I was on was the Admiral Evans.

Q. How long were you on it? A. I was on it about seven months.

10451 Q. That is, you began service on the Admiral Evans when? A. About the early part of October, somewhere the first part of October.

Q. Was that installed with the Thompson transmitter that was in use last winter, February? A. Yes, sir.

Q. At the time you went on it? A. Yes, sir.

Q. You are positive about that? A. I am positive about that.

Q. That same transmitter was installed before the time you went on the Admiral Evans? A. I do not know when it was installed, but it was installed when I was there.

10452 Q. It was installed when you went to work there? A. Yes, sir.

Q. Was there any change made in it after you came there, in any respect? A. There was not any change made on that set from the time I came aboard until I left.

Q. Any kind of a change? A. No kind of a change, no change whatsoever.

Q. No part of it whatever was altered or substituted at any time? A. No, sir.

Q. What boat were you on prior to that time? A. Prior to that time I was on the—it was the Zaphora.

 10453

Q. You say the Zaphora was the one you were on before that, before the Admiral Evans? A. Yes, it was the Zaphora I was on.

Q. Before leaving the Admiral Evans did you ever carry any extra spark gaps on the Admiral Evans? A. Why, not that I know of.

Q. Are you sure you did not have extra spark gaps? A. I think there was two spark gaps in the lower drawer, but they were never used. I do not know whether they were used or not. We never changed them or had any thing to do with them.

Q. What kind of a spark gap was it? A. It was the 10454
same kind of gap as it had installed.

Q. The same kind they had installed? A. Yes.

Q. And that was what kind of a gap? A. That was a quenched gap.

Q. Now, how long were you on the Zaphora? A. I was on her about six weeks.

Q. That was in September, or the last of August and the month of September? A. Yes, sir, approximately that.

Q. You changed directly from the Zaphora to the Admiral Evans? A. Yes, sir. I was laid ashore for awhile and then went over to the Evans.

Q. Was the same kind of a transmitter used on the 10455
Zaphora as was used on the Admiral Evans? A. Yes, sir, the same kind, except we did not have the storage battery, did not have the auxiliary power.

Q. Was it the same size apparatus? A. Yes, sir, one kilowatt.

Q. The same size of loop? A. Yes, sir, the same size loop.

Q. You say you never saw but the one loop on either of those boats? A. On every boat I worked on——

Q. I will ask one question at a time, just answer me;

was it the same on those two boats? A. Yes, sir, the same two boats.

Q. Did you measure the diameter of the loop? A. No, I did not.

Q. What boat were you on before the Zaphora? A. On the Star.

Q. When were you on the Star? A. Well, I was somewhere around in August, I guess, the month of August.

Q. How long were you on the Star? A. About three weeks.

Q. What sort of transmitting apparatus was on that,
10457 was it precisely like that on the Admiral Evans? A. Yes, sir, with the exception of not having storage batteries, one kilowatt.

Q. With that exception there was no difference whatever? A. No, sir, there was no difference whatever.

Q. What were you on before the Star? A. I was on the tug Tatoosh.

Q. How was that installed, with the Thompson transmitter at that time? A. Yes, sir.

Q. Was it precisely the same kind of a transmitter as that on the Admiral Evans? A. Yes, sir, it was.

Q. In all respects? A. Yes, sir.

Q. Were you ever on the Alameda? A. I never was
10458 on the Alameda.

Q. Were you ever on any other ship? A. No, sir—oh, I have been on other ships. I have been on Marconi ships.

Q. I mean any other ships carrying the Thompson transmitter type. A. No, sir, that is all the ships I have been on.

Q. Have you ever seen what has been spoken of as the old or original Thompson type, as distinguished from the new or later Thompson type? A. No, sir, I have not.

Q. Do you know what the use of that loop was? A. Yes, sir.

Q. What? A. Why, it was one loop of inductance.

Q. A loop of inductance? A. Yes, sir.

Q. Do you know what the meaning or purpose of inductance is? A. Yes, I have got an idea what it means.

Q. Have you examined the condenser? A. No, sir.

Q. You never took off the cover? A. No, sir, I never did.

Q. You do not know anything about that? A. No, sir, I do not know anything about that.

Q. You say you never made any tests; you mean to say you never made any changes of fixed things, not that you did not use any of the functions that were afforded you there to make changes that could be made in altering wave-lengths or anything? A. Of course that is natural, operators have to make them changes. That is part of the set there. 10460

Q. Do you know how the loop was connected to the condenser? A. Yes, sir, I have an idea.

Q. Tell us. A. Well, if you showed me a diagram of it, I could give you an idea whether it was right or not.

Q. You have spoken of using a condenser with the receiver; what condenser did you use? A. A variable condenser in the receiver. It might be a fixed condenser there also, but I am not sure of it. 10461

Q. You have spoken of transmitting and hearing signals? A. Yes.

Q. Did you work with any Kilbourne & Clark land stations? A. Kilbourne & Clark land stations?

Q. Yes. A. I didn't know they had any land stations.

Q. What land stations did you work with? A. I worked the station at the port of Seattle, at the dock down there.

Q. Is that the only land station? A. Yes, sir, that is the only land station I have worked.

Q. When you speak of hearing, you do not mean that you communicated with North Head or any of those other places? A. No, sir.

Q. You simply heard the signals? A. I simply heard the signals.

Q. By listening in with your receiver you could detect them? A. Yes, sir, hear the signals.

Q. Do you know the size of the loop on the Star? A. Yes, sir, I have an idea what the size of the loop is on the
10463 Star.

Q. What? A. It is just like the rest of the loops.

Q. That was not quite the question I asked you. I asked you if you knew its size? A. No, sir, I do not know the size of the loop. I never took a measurement of it.

Q. (Mr. Skeel) Mr. Lipke, you spoke of the variable condenser in the receiver; will you please state what circuit that was in? A. That was in the primary circuit, or the antenna circuit.

(Witness excused.)

10464

Walter Rathbun, produced as a witness on behalf of Defendant, being first duly sworn, testified as follows:

Q. (Mr. Skeel) State your name? A. Walter Rathbun.

Q. Please give your place of residence and your occupation. A. Kirkland, Washington, is my place of residence.

Q. What is your occupation? A. Radio operator.

Q. How long have you been a radio operator? A. For over a year.

 10465

Q. Have you worked on any boats installed with the Kilbourne & Clark radio apparatus? A. Yes, sir.

Q. Transmitter and receiver? A. Yes, sir.

Q. What boats have you been on with that apparatus? A. On the steamer Star and on the Kansas City and on the Santa Ana, and the tug Henry J. Biddle.

Q. Will you please state, Mr. Rathbun, whether on any of the boats that you have been on there have been extra loops or primary inductance for the transmitter, or whether there have been any variable extra condensers in the secondary of the receiver? A. No, sir, there have been none. 10466

Q. Will you state whether or not you have ever received signals over long wave-lengths? A. Yes, sir.

Q. State over what distance with the Kilbourne & Clark receiver? A. (Referring to memorandum) While on the Kansas City at Latouche, Alaska, I heard the U. S. Government Naval Station at Eureka, California, use a wave-length of approximately 1800 meters, a distance of about 1450 miles.

About the same time I heard the U. S. Government Naval Station at North Head, Washington, on a wave-length of about 1800 meters, a distance of about 1200 miles. 10467

At about the same time I heard the U. S. Government Naval Radio Station at Tatoosh Island, Washington, on about 1800 meters, a distance of about 1100 miles.

On the same ship, off Fire Island, in Cook's Inlet, Alaska, on November 19th, at about 11:20, I heard the U. S. Government Naval Radio Station at North Head, Washington, on about 1800 meter wave-length.

Q. What distance was that last? A. I judge the distance is about 1500 miles, approximately.

Q. Is that all? A. I have others, but I have no definite data at hand.

10468 Q. I notice you are reading or refreshing your memory from a memorandum; will you please tell the court what that is. A. I copied this from my daily log off the Kansas City.

Q. On the steamship Kansas City? A. Yes, sir.

Mr. Skeel: Cross examine.

Cross Examination.

Q. (Mr. Hughes) Will you let me see that memorandum. Speaking first of these observations, you
10469 mean simply that you listened in at the times when you say you heard these various stations transmitting? A. Yes.

Q. What time of the year was that? A. It was in the fall of 1915.

Q. For instance, at the time when you say you heard Government Stations, the Government Naval Radio Station at Eureka, California? A. That was in the fall.

Q. Well, what time in the fall? A. In the month of November.

Q. Was it in the day time or at night? A. At night.

Q. What time at night, about what time? A. It was
10470 before midnight, between nine and eleven, approximately, I believe, if I remember right.

Q. What means did you have of knowing how many meters the wave-length was that the Government Naval Station was using? A. The only means I had of knowing was by the amount of inductance I had on my tuner.

Q. How did you determine it? A. Judging from the setting I had over a 600 meter wave-length. It was a great deal above the 600 meter wave-length.

Q. What is it? A. The setting I had for the naval radio station was a great deal above the 600 meter setting.

 10471

Q. Your own wave-length, your own setting, was 600 meters? A. It was a setting I used for receiving ship's stations, about 600 meters. The set I used for receiving the naval radio station was a great deal more than that.

Q. You have no way of doing anything but guessing at it? A. That is all.

Q. In other words, you had no way of adjusting your receiver by condensers or by any other method to change the wave-length received, did you? A. I do not quite understand.

Q. What I want to know is if you had any way of setting or adjusting your receiver to a wave-length—the 10472
only method of adjustment is in the primary receiver, isn't it? A. The only method is the antenna inductance, yes, sir, antenna inductance, primary inductance.

Q. And you had no way of computing the other except by guessing at it? A. Yes, sir.

Q. You speak of hearing the radio station at Tatoosh; what time of the year was that? A. That memorandum refers to the same time of the year and the same time of day.

Q. The same time of night? A. Yes, sir.

Q. And the same thing is true in regard to the length of the wave meter of the Government Station at Tatoosh; 10473
that is to say, you guessed at its wave-length. A. That was the same as the station at Eureka and at North Head.

Q. And then you speak of hearing the Government Radio Station at St. Paul Island, Alaska? A. That was also at the same time of the year and the same time of the night.

Q. And you formed your judgment of the wave-length, of the wave meter length, as you did in the other instances? A. Yes, sir.

Q. You also spoke of hearing North Head, that occurred about the same time of the year and the same

10474 time of night, about midnight? A. Yes, sir. That was from what point?

Q. North Head. A. What point was the receiving point?

Q. Well, you only mention one instance where you heard North Head? A. I think I mentioned two instances there.

Q. I do not find but one noted here, and in order that you may not be confused at all I will read just what you have: "On steamship Kansas City off Fire Island, Cooks Inlet, Alaska, November 19th, at 11:20 p.m.; heard U. S. Government Radio Station at North Head."
10475 That is the time that I am referring to. A. That is about the same time of the year.

Q. And the same time of night? A. About the same time of night, yes, sir.

Q. Your estimate of the wave meters used from North Head was made by you in the same manner as in the other instance? A. Yes, sir.

Q. Now, how long do you say you have been in service as a radio operator? A. For over a year.

Q. What was your first boat? A. The first boat was the President—you mean using Kilbourne & Clark apparatus?

10476 Q. Yes. A. The Star.

Q. You had had experience before you used the Kilbourne & Clark apparatus? A. Yes, sir.

Q. On what boat? A. The President, and the Admiral Watson.

Q. When did you go on the Star? A. I forget the exact time. It was in the fall of 1915.

Q. Do you remember whether you were on before or after Mr. Lipke? A. I was on just after Mr. Lipke.

Q. How long did you say you were on the Star? A. Approximately six weeks.

 10477

Q. And was it after that that you were on the Kansas City? A. Yes, sir.

Q. Was the Kansas City equipment the same as that on the Star, the transmitting equipment? A. No, sir.

Q. It was different? A. Yes, sir.

Q. Which was the newer type? A. The transmitter on the Kansas City.

Q. When did you go on the Kansas City? A. A short time after leaving the Star.

Q. Well, perhaps you have told me, but I do not recall how long you were on the Star? A. About six weeks.

Q. Was it about December that you went on the Kan- 10478
sas City? A. Yes, sir.

Q. And it must have been in November, because I see by your notes you were on the Kansas City in November? A. Yes, sir.

Q. So that you must have been on the Star in October? A. About then, yes, sir.

Q. What was the difference in the transmitter on the Star from that on the Kansas City? A. The Star was equipped with a Thompson transmitter, and the Kansas City was equipped with a Simpson transmitter.

Q. The Kansas City had the Simpson transmitter? A. Yes, sir.

Q. Then you have not meant to testify to the use of 10479
this loop on the Kansas City? A. No, sir.

Q. What was installed on the Biddle tugboat? A. Approximately the same kind of a set as on the Star.

Q. What do you mean by "approximately"? A. Well, as far as I know it was the same.

Q. Do you know of any new type of the Thompson transmitter, any new, more modern construction of the apparatus; do you know any difference between them? A. No.

Q. When were you on the Biddle? A. During May and part of June of this year.

10480 Walter Rathbun—Cross.

Q. Do you know when the Biddle was equipped? A. Yes, sir.

Q. When? A. It was equipped in the early part of May or the latter part of April.

Q. Of this year? A. Yes, sir.

Q. Now, you had then been an operator how long before you first went on the Kilbourne & Clark apparatus? A. Two or three months, I should judge.

Q. What was the last boat you were on before you went on the Star? A. The Admiral Watson. The last boat I was assigned to was the Paraiso, but I did not
10481 sail on it.

Q. I understood you that the tug Biddle was installed this last spring? A. The spring of this year.

Q. The first boat that you went on that had the Kilbourne & Clark apparatus was the Star? A. Yes.

Q. What boat were you on before that? A. I was assigned to the Paraiso.

Q. No, but what boat were you radio operator on before that—you did not operate on the Floridan, did you? A. I did not operate where, sir?

Q. You say you were assigned to some boat? A. Yes, I was assigned, but never sailed on it.

Q. I was not asking about that, I asked what boat
10482 you were operator on. A. The Admiral Watson.

Q. What equipment did that have? A. Marconi.

Q. And all your experience prior to going on the Star was on boats having the Marconi apparatus? A. Yes sir.

Q. Did you go directly from that experience to the Star and operate the Kilbourne & Clark apparatus? A. Yes, sir.

Q You did not require any new training or education? A. I was shown how to operate the set by one of the former operators on the Star.

Q. Well, that was merely the time it would take him

 10483

to show you, just enough to point out the instrument and the method of working it, wasn't it? A. Yes, sir.

Q. Were you on any other vessels than the Star and Kansas City and the Biddle that carried Kilbourne & Clark transmitters? A. Yes sir.

Q. What was it? A. The Santa Ana.

Q. When was that? A. It was previous to going on the Biddle.

Q. Do you know when the Santa Ana was installed? A. No sir.

Q. Was the transmitter on the Santa Ana the same as that on the Kansas City? A. No sir. 10484

Q. What was the difference? A. The Santa Ana had a Thompson transmitter and the Kansas City did not.

Q. Was it the same as that on the Star? A. Yes sir. It had storage batteries in addition.

Q. Was it a newer or older type than the Star, do you know? A. So far as I know they were the same type.

Q. Did you ever measure the size of the loop on either or both of those vessels, the Star or the Santa Ana? A. No sir.

Q. Did you ever examine the parts of the transformer? A. No sir.

Q. So as to know whether you could change its inductance in any way? A. Did I understand you to say the transformer? 10485

Q. I did not mean the transformer, I meant the transmitter. A. The only way I examined it was by looking at it from the outside.

Q. Do you know what the inductance consisted of? A. I think I do.

Q. Of what did it consist? A. As far as I could see from the outside it consisted—in what circuit?

Q. In the transmitting primary circuit. A. The

10486

primary circuit consisted of transformer, spark gap, condenser and primary inductance.

Q. I asked you what a primary inductance consisted of? A. It consists of one turn of copper tube.

Q. A loop? A. Yes, sir.

Q. And the wire connections with the condenser? A. Yes, sir.

Q. Of what did the condenser consist? A. I cannot say of my own authority.

Q. Did you ever have the cover off of it? A. Yes, sir, I have looked at it with the cover off.

10487 Q. Of how many condensers was it composed? A. There are two banks of condensers in series, I believe.

Q. Any way of separating the parts, or adjusting the amount of capacity in the condenser? A. No, sir.

Q. How? A. No, sir, they are fixed.

Q. There was not any way then at all on that condenser by which you could do that? A. No, sir.

Q. Well, could you cut any part of it out of the circuit or into the circuit? A. I could not do it as an operator. It could be done in the shop, I suppose, but not by the operator.

Q. I am talking about on the boat? A. No, you could not.

10488 Q. Speaking of these government stations; do you know anything about what power they use at those government transmitting stations? A. I don't know about that.

Q. You know they are of very great horse power? A. I don't know what the power is.

Mr. Hughes: That is all.

RE-DIRECT EXAMINATION.

Q. (Mr. Skeel) Mr. Rathbun, with reference to the in-

 10489

structions you got in the use of the Kilbourne & Clark receiver; will you please state comparatively, how many adjustments there were on that receiver, as compared with the previous Marconi receiver which you had used? A. On the Marconi receiver there was means to adjust——

Q. Just the number, so as to save time, if you can give it to me? A. I think there is two on the Marconi, or three on the Marconi, and two on the Kilbourne & Clark.

Q. (Mr. Skeel) Solely from your own instrument, how could you tell, approximately, what wave length was 10490
being received? A. I was using the greater part of my antenna inductance at the time.

Q. And was there anything there to indicate when you are using 600 meters, or when you are receiving 600 meters? A. Nothing to indicate it exactly.

Q. Nothing to indicate it exactly—from your experience are you able to tell when you are receiving 600 meters? A. Yes.

Q. And how could you estimate when you are receiving a larger or higher wave length in comparison to the 600 meters? A. By cutting in more inductance than 600 meters.

10491
Q. How can you know how much additional inductance, approximately, would bring it up to 1800 meters? A. I know approximately, exactly, what the antenna inductance is, from the tuning chart, and I know about how many turns of inductance I have when I am using 600 meters. I know how many turns of inductance I have when I am receiving for the longer wave length.

Q. What I am trying to get is; is it merely a guess that you are receiving these larger wave lengths, or do you know from your experience as an operator? A. I know from experience.

10492

Q. (Mr. Hughes) You think that if you have three times as many turns in your primary, that the wave length you are receiving is three times as great; is that what you mean? A. Not exactly. The antenna itself has to be taken into consideration.

Q. But aside from that, that would be your conclusion, would it? A. It would not be exactly so, no—just approximately.

Mr. Skeel: That is all.

(Witness excused.)

10493

R. E. Thompson, produced on behalf of the defendant in surrebuttal, testified as follows:

Q. (Mr. Skeel) Mr. Thompson, you have been already sworn in this case to testify as a witness? A. I have.

Q. Did you attend the plaintiff's tests of the Thompson impulse transmitter in the presence of the assessors in the month of April, 1916? A. I did.

Q. Will you please describe those tests and state what was shown as the result of them? A. Why, a series of tests were made by the plaintiff, in which the regular
10494 loop inductance built into the defendant's standard impulse transmitter was changed to something else. They substituted various loops, such as this loop over here on the end of the table.

The Court: Exhibit number——

A. (Showing)—exhibit 44—such as exhibit 44, and noted what results the substitution had upon the relative efficiency of the transmitter in the transfer of its energy from one circuit to another. They first put in, if I recall the order in which they were substituted—they put in a small loop and measured the antenna current, that is the current in the other circuit to which this loop was primarily associated; and then they put in a larger loop and then measured the current, and then they put in a still larger loop and measured the current. And then they measured the current with the regular loop which we supply with the transmitter, and then they added larger loops than the one which we regularly supply with the transmitter, each time making a measurement of the current in the antenna circuit.

The purpose, as I understood them to testify afterwards, being to show that the further out of tune the 10496
two circuits, that is, the impulse charging circuit and the antenna circuit, the less the radiation, and the nearer in tune, the greater the radiation. And I noted, however, that those tests were run backwards, so to speak, from any such tests as engineers ordinarily make to determine such things. As you see, ordinarily with a fixed circuit, such as defendant's impulse charging circuit is, if it is desired to find out what the radiation on the different wave lengths will be in a given antenna circuit, an antenna circuit which is by nature a variable circuit, and intended to be a variable circuit, but which does not have its characteristics changed by varying it—
10497
would ordinarily be varied and the wave length to which it is adjusted, and the relation of those wave lengths to which it is adjusted, noted, leaving the impulse charging circuit in its normal operating condition. However, instead of that, the plaintiff's witnesses reversed the order of things and——

The Witness (Continuing): So, instead of varying the antenna circuit, which is a variable circuit, and leaving the fixed circuit in its fixed condition, they reversed the order of things, and

10498

fixed the antenna circuit and they varied the closed or impulse circuit.

The Court: Varied what?

A. (Continuing) They varied the closed or impulse circuit, as they term it, and in that way they made those measurements which they claim show a tuning to a certain extent between the antenna circuit and the impulse charging circuit. That, in my mind, would certainly spoil any deductions which might be made from the tests as they were run. However, there was a noteworthy result, which was very plain throughout all the tests; that
10499 was that, regardless of how this impulse charging circuit was abused or changed or substituted or made over, it still remained an impulse charging circuit; that is to say, it did not have anything to do with the wave length being radiated in the antenna circuit.

A. (Continuing.) Now, as the antenna was changed, or as the impulse charging circuit was changed by the substitution of these various loops, ordinarily had that change caused that impulse circuit to become an oscillatory circuit, it would have caused two waves to have shown up in the antenna circuit,—it would have caused a reaction between the two, which would have resulted
10500 in a complex wave form in the antenna circuit, which would have shifted around to different positions, according to whichever loop was being used. However, instead of that happening, the wave length in the antenna remained absolutely fixed at 600 meters, or as near thereto as they could measure; while the wave length in the impulse charging circuit was changed over a very wide range, from something away down below 600 meters, to something up around 1200, as I remember. And this showed, as I stated before, that the impulse charging circuit has no effect on the antenna circuit one way or the

 10501

other, and it incidentally shows the remarkable difference between this type of transmitter and that type of transmitter which might be called the coupled tuned circuit type.

Mr. Betts: I object to this as argumentative, if the court please and not surrebuttal.

The Court: Yes; I think he is getting a little into the matter of argument a little more than necessary. Please confine your testimony to conclusions in the nature of experiments rather than a comparison with others. 10502

(Mr. Skeel.) Please compare the characteristics of the waves in the spark circuit and in the antenna circuit of the Thompson transmitter, as shown by the plaintiff's tests, with what you have stated to be the coupled tuned transmitter? A. I will make a diagram.

Q. To save time, can you use any of these? A. I believe I can if I can find one that applies.

Mr. Hughes: If the court please, that is calling for expert testimony and it is not rebuttal. If this is permitted, we should certainly, in fairness, be permitted to answer it.

The Court: I think it is proper now to show 10503 whether the demonstrations made were such as were fair and demonstrated the results which the apparatus is supposed to perform.

Mr. Skeel: It is our contention that this test conclusively proved our contention in this case. Mr. Thompson was the only witness that we had present at those tests.

The Court: He may answer.

Mr. Skeel: Please give the number of the exhibit?

A. G. W. P. 38 and G. W. P. 32.

Mr. Betts: This question is not directed to the assessor's tests, but to a comparison of the Thompson transmitter with some other transmitter which is not a part of the tests conducted by the assessor, which this witness is supposed to be talking about.

The Court: The inquiry should be applied to the assessors' tests, and I understood it was.

Mr. Skeel: Let me state exactly what will be said; these tests were for the purpose of proving that this transmitter is a coupled tuned trans-
10505 mitter.

Mr. Betts: I beg your pardon.

Mr. Skeel: Now, it is our contention that those tests, on the contrary, proved that it was an impulse transmitter. The witness will illustrate what the tests would have shown and what the results in the wave length in the antenna, had it been a coupled tuned transmitter, as plaintiff alleges, and in order to do that it is necessary to show the difference in the antenna wave trains between the two types of transmitters.

Mr. Hughes: It must be perfectly clear to the
10506 court that if this is permitted to go in—of course the court has discretion as to the order of proof—but if it is permitted to go in, being the first of its character, we would clearly be entitled to answer it.

Mr. Skeel: Why, it must be the first of its character, because we had no opportunity to testify as to these tests.

The Court: Proceed. I must know whether the test to which the apparatus was put was a proper one and a fair one; that is the purpose, and this is the first time that these parties had an opportunity to be heard on that.

Mr. Hughes: This is an attempt to compare it with something else, or an imaginary thing.

The Court: No, I understand this is a comparison of the test that was applied to it, and not some other test, but the test that was applied by the assessors, I understand.

Mr. Skeel: That is exactly what it is.

The Court: Proceed.

Q. Just limit that as carefully as you can, Mr. Thompson, to make the point clear. A. In order to make the tests that were conducted by the plaintiff, clear, we need to compare it with something else. It is only by comparison that an impulse transmitter is called an impulse transmitter; and the energy—referring to G. W. P. 38—(Vol. 3, p. 1635) the energy of the impulse charging circuit may be represented by the black line, showing approximately two oscillations. 10508

Q. At the top of the chart? A. At the top of the chart.

Q. On the left hand side? A. The left hand side. The energy in the antenna circuit may be represented as the long drawn out red line. The point that the tests showed conclusively was that the relation of this wave length as represented by this line—— 10509

Q. What line—the black line? A. The black line—its relation to the wave length represented by the red line are independent of each other in this type of transmitter. If this had not been the case we would have immediately got a different result altogether, which would have shown itself in the form of the curves drawn in the chart G. W. P. 32 (Vol. 3, p. 1617).

Q. At the top of the chart? A. At the top of the chart.

The oscillation curves shown in c show in the impulse charging circuit or the primary circuit, or whatever the first circuit may be called, a series of oscillations——

Q. Indicated by the black line? A. Indicated by the black line; and in the antenna circuit there is a series of oscillations indicated by the red line. This shows a continual trading backwards and forwards between the two circuits, and the energy first in one circuit and then in the next, and then back to the first and then in the second circuit and so on, until part of it is consumed in heat and part of it has been radiated; but that part of it which has
10511 been radiated has been radiated in two different wave lengths; not one pure wave length but two separate wave lengths being radiated from the same antenna, neither one of those wave lengths being, necessarily, the same wave length as the antenna itself, but some different wave length, one usually above the natural wave length of the antenna and the other below the natural wave length of the antenna. And it is only when we have a pure impulse excitation, or some means of preventing those two waves from being radiated that you have a result as shown in the chart G. W. P. 38, where, at the black line representing the primary circuit, the oscillations exist for an extremely short period and are then cut off, permitting
10512 the antenna circuit to oscillate freely and not trade its energy back to its primary circuit, as in the other case.

Now, my experience in investigating just such conditions as this—I was appointed radio inspector when the laws first went into effect—one of the government's radio laws was aimed at the exact condition I have been speaking of—the elimination of this trading backwards and forwards of the energy from one circuit to another, and thereby creating two wave lengths in the antenna circuit, one of which wave lengths could not be used, but must be, necessarily, sent out into the atmosphere and

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thereafter interfere with anybody who might want to work on that same wave length. In other words, it served no useful purpose, and might serve a very harmful purpose. So, in 1912, a law was passed, the sum of substance of the part which appertains to these particular tests, being that the wave must be both sharp and pure. Sharp meaning that it must have a great number of oscillations, and pure meaning that those oscillations must not be traded back into the primary circuit and thereby causing two wave lengths to be radiated from the antenna circuit.

Q. Referring to G. W. P. 38, the waves indicated in the 10514
red, does that represent substantially, the free oscillations in the antenna? A. That does represent free oscillations in the antenna, if the oscillations were not free this would be a rising and falling value, instead of a gradually declining value.

Q. The black line on G. W. P. 32, representing the waves, in the primary circuit, is that purely a theoretical condition, Mr. Thompson, or was that a matter of actual practice in the radio art prior to 1912?

Mr. Betts: I object to that as not proper surrebuttal.

Mr. Skeel: Mr. Weagant stated yesterday that 10515
he had never seen such a transmitter.

Mr. Betts: I beg your pardon.

The Court: Well, he may answer.

The Witness: This type of wave was present in the primary or persistently oscillating primary circuit of all the transmitters in universal use. I do not mean absolutely all transmitters, but the type of transmitter usually found on board ships sent out such waves as this and, as I said before, it was for that reason that the law was enacted. It was to cure that condition, and if that had not been the universal practice it would not have been enacted.

Mr. Skeel: Now, I wish to state to the court that in view of Mr. Betts' last objection, I am going to read Mr. Weagant's testimony.

Mr. Betts: If it is in the record, that is sufficient.

Mr. Skeel: But I object to mis-statements of what the record was yesterday; Mr. Betts knows that Mr. Weagant made that statement yesterday

10517 and for the first time.

Mr. Betts: Just read it.

Mr. Skeel: I will read it as soon as I find it.

Q. Now, Mr. Thompson, did you attend the plaintiff's receiver tests in the presence of the assessors at the L. C. Smith Building in April? A. Yes, I attended all those tests.

Q. Will you please describe what those tests were and what they showed? A. Three series of tests were run by the plaintiff's witnesses, and the first series of tests being to compare defendant's standard receiver in its present or natural untunable condition with the receiver changed

10518 over into a tunable one. That is to say, tuning means were provided and connected to part of the circuit and a switch was provided for connecting this tuning means on and disconnecting them, and a series of readings on the telephones were made for different wave lengths, varying from 3600 meters down to 300 meters, and the relative strength of these signals was noted when the tunable means were used and adjusted to exact resonance, and when those means were disconnected and not present. Then a similar series of tests were run, where the same thing was done over again, except that instead of the tele-

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phone receivers being used to indicate the strength of the signals, a galvanometer was used—a galvanometer being a device which would visualize the signals, you might say, and thereby you would have something substantial to go on, something which you could read and make notes on the relative values—the scale on the galvanometer being calibrated into degrees, and the deflection of the galvanometer could be noted and records would be made of the comparative strength of the signals when the receiver had tunable means connected to it and close adjustments, and when those tunable means
were absent. 10520

Then another series of tests were run to where a wave meter was set up, and different wave lengths were sent out from this wave meter, and it was noted which spots on the crystal detector corresponded loudest to different wave lengths; and I might say that no particular spots were pointed out. They would simply adjust the detector around until it apparently responded to a little longer wave length slightly louder than had been responded to on the shorter wave length. And those records were noted, and the assessors duly made their reports.

Throughout those tests I was present, and from time
to time I asked for certain checking-up tests. I did not 10521
have any particular idea as to what they were doing, and the assessors did not know what the object of the tests was—what they intended to show by them—but I requested that I be permitted to check up whatever little variations there were, by manipulating the instruments myself, as I consider that one can get any results you are after if one makes all manipulation of the instrument. As, for instance, down in the radio inspector's room, if I had manipulated the Marconi instrument and the defendant's instrument also I could have secured any ratio of efficiency between the two that I might have seen fit,

either purposely or due to my lack of knowledge of how to manipulate the plaintiff's receiver.

I asked for checking-up tests, which I was requested to put over until they had completely finished, at which time I might be permitted to make them. However, they took their instruments away as soon as they finished the tests, so that I did not have the opportunity, and I purpose to make those tomorrow; the object being to show that certain conclusions that the plaintiff's witnesses drew from the results of those tests, had no foundation in fact.

Q. Mr. Thompson, please state whether or not the
10523 results of the receiver test indicated that the receiver is partially tuned? A. They most certainly do not indicate anything at all as to the natural condition of the defendant's receiver, so far as tuning is concerned. The tests indicated that so long as they provided or adjusted the coupling between the two circuits, I might say, the detector circuit and the antenna circuit; closely enough to each other; or, in other words, so long as the desired degree of coupling was maintained, the signals not only could not be received louder by tuning, but that if any tunable means were added, you could get the signals as loud. They further show that when his coupling was not maintained at the desired degree and it was impossible
10524 to so maintain it on the longer wave lengths, that then one might resort to another type of receiver and receive those same signals without the close coupling.

That is to say, the coupling of that particular receiver is limited by the two coils approaching each other and when they come in actual contact you cannot get them any closer together—they stand end to end—and from that on you cannot increase your coupling, so far as physically placing those coils closer to each other. However, you decrease your coupling by increasing your wave length, because part of the inductance which is included in the

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antenna to increase the antenna wave length is not closely associated with the detector coil, and for that reason as you go up on your wave length that is as you increase the wave length in the antenna circuit then you have the actual effect of loosening your coupling, which cannot be compensated for by pushing the detector coil closer to these newly included coils which have been placed in the antenna circuit.

Those tests show conclusively that that was the case; and that if the coils could have been placed close enougn together, or one placed inside of the other, that the actual
results would have been on the longer wave lengths you 10526
would receive signals louder, without any tunable means, than with the tunable means.

Q. Just state in a word the purpose and effect of the coupling between the primary and the secondary of the receiver? A. The coupling between the primary and the secondary of the receiver determines the amount of potential that is developed across the detector, and you must always connect your detector very closely to the circuit from which it receives its energy, so that all the potential of that circuit will be developed across the detector. Where you only have a two circuit receiver,
that is one oscillatory circuit and one non-oscillatory, 10527
your non-oscillatory circuit must be more closely associated with your antenna or oscillatory circuit in order that the energy may be directly transferred from the antenna into the detector circuit. However, if for any reason you wish to be able to disassociate those two circuits a great distance from each other, you may introduce a third circuit. You can put in a second oscillatory circuit. That can be very easily done by sticking a condenser into the detector circuit; building one in there, or so constructing the detector circuit that it will contain a condenser—a variable condenser—and in which case you

then make your detector circuit into two circuits—divide it, so to speak. The newly introduced circuit is an oscillatory circuit. It no longer has to be closely associated with the antenna circuit, because it can receive its energy a little at a time by the process of building up, due to sympathetic resonance. If you tune those two circuits together, however, in the latter case you have then your detector very closely associated with your second oscillatory circuit. In every case you must associate your detector closely to the circuit from which it receives its energy. Therefore, if you eliminate your second oscilla-
10529 tory circuit you must immediately push your detector close up to the antenna circuit if you expect to cause its energy to be received. That is the reason why the introduction by the plaintiff of a second oscillatory circuit into the defendant's receiver permitted the energy to be received without a close coupling. If they had provided a close coupling for the receiver then the introduction of this second oscillatory, or tunable circuit would not only have not helped matters any, but would actually have hindered them.

Q. State what the fact is with respect to the closeness of coupling when the receiver was receiving wave lengths of 600 meters and when it was receiving wave lengths
10530 of higher lengths, as much as 1200 or 1800 or above? A. There was nowhere near the same degree of coupling at the wave lengths of above 600 meters as there was when the wave lengths was at 600 meters or less. The point where they seemed to be unable to get a close enough coupling began about eight or nine hundred meters, if I remember correctly. From that on up the inductance whichwas included in the antenna circuit was so far removed from the detector circuit that it did not have any material effect. The amount that was included in the antenna circuit when wave lengths of 2400 meters or

longer were received, as I remember it, was not associated with the detector at all. It was in another coil, this other coil being placed at right angles to the detector coil; and it is well known in the art that if you place two coils at right angles to each other they will have no effect upon each other. They must be placed in the same plane.

Q. Was this coupling closer or not, as the wave lengths increased? A. It was very much looser.

Q. And what effect would that have on the intenseness of the signals? A. It would decrease them just in proportion to the looseness of the coupling.

Q. Now, do you recall Mr. Weagant testified that if 10532
the detector circuit had a natural period of 200 meters, that the receiver would not be commercially operative? A. Yes, I recall that testimony.

Q. What is your opinion as to that? A. My opinion based on the actual construction of such a coil, is that it will not only respond to a wave length of 600 meters efficiently, but it will respond to a wave length of 3600 meters, and still more efficiently than a coil constructed having a longer period.

Q. Mr. Weagant stated that the receiver of the defendant in this case was broadly tuned to a range of wave lengths between three and six hundred meters. Please
state what your opinion upon that statement is? 10533

Mr. Betts: I don't think you have quoted this testimony correctly.

Mr. Farnsworth: That is what he said, and Mr. Waterman said so also.

Mr. Betts: He said "the secondary of the receiver," and not the receiver.

Mr. Skeel: I will insert the word "secondary."

(Question repeated to the witness as follows:)

"Q. Mr. Weagant stated that the secondary of the receiver of the defendant in this case was

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broadly tuned to a range of wave lengths between three and six hundred meters. Please state what your opinion upon that statement is?"

A. Well, just what Mr. Weagant meant by "broadly tuned," of course, I do not know; and my interpretation of "Broadly tuned" circuits is that it means that they are not tuned at all. It might be construed to mean that they were in the same general neighborhood of each other, but to say that one circuit is broadly tuned to another is equivalent to saying that it is not tuned to that circuit at all. If you want to tune one circuit to another
10535 you tune it; that is, you adjust it to have it just exactly the same natural period—a period of electrical oscillation; that is to a degree where you cannot notice any difference. If you have all the means available for making those adjustments, and if you have an adjustable or tuned circuit, and if you want to tune one to the other, you tune them to where you cannot note any difference; or where a slight change will make a noticeable difference; then you are in tune. If you move them over to where it makes a very noticeable difference, it is very conclusive proof that you are out of tune; if the effect you have been getting has been dependent upon tuning. Of course, you can get the maximum effects in the adjustment of certain
10536 things without it being due to tuning alone; but where your effect is due to tuning, the point you wish to bring out is—so long as you have the adjustable means and are using them at all—it is just as easy and easier to put them exactly in tune than some other degree out of tune; and to say that two circuits are broadly tuned means nothing to me, except that they are not tuned at all.

Q. Now, summarizing the plaintiff's receiver's tests in the L. C. Smith Building; please state your conclusion, as to whether or not the results obtained on the higher wave lengths show any substantial benefit due to reson-

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ance, or was it a matter of coupling? A. They did not show any advantage whatever due to resonance. They did show that with a closer coupling than was being used, a greater efficiency would have been obtained.

Q. And has the matter of coupling anything to do with the time period of a circuit? A. Well, that is a matter independent of the time period of the circuit.

Mr. Skeel: That is all.

Mr. Betts: Does the court wish me to proceed now with the cross examination; because they are going to recall this witness to describe the tests which they are going to make tomorrow, and it will 10538
mean interrupting the cross examination.

The Court: Do you want the witness again?

Mr. Skeel: Just briefly, for the purpose of describing the new tests.

The Court: Have you any other testimony?

Mr. Skeel: Yes; we can qualify Dr. Zenneck.

The Court: Perhaps it will save time.

Mr. Betts: I think it would be a little more orderly, that is all.

(Witness excused.)

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Jonathan Zenneck, produced as a witness in behalf of defendant in surrebuttal, being first duly sworn, testified as follows:

Q. (Mr. Farnsworth.) State your residence and occupation? A. I am residing in Munich, Germany. I am a Professor of Experimental Physics at the so-called Techniche Hoch Schule at Munich, which corresponds to the Technical Departments of the American Universities or to the Institute of Technology.

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Q. Were you present on July 3rd and 4th last at the plaintiff's Massachusetts tests of the Simpson mercury valve transmitter? A. I was.

Q. Will you please state your experience and qualification which enable you to testify concerning those tests which you witnessed in Massachusetts? A. I had been, from 1895 until 1904, assistant of Professor Braun, the inventor of the Braun tube which had been used in those Massachusetts tests, and the inventor of the Braun transmitter. In 1899 and 1900 I was engaged in radio telegraphic experiments with the Braun transmitter on the
10541 North Sea.

I have published two books on wireless telegraphy. The first was issued in 1905. The title is, as translated in English "Electro Magnetic Oscillations and Wireless Telegraphy." This book has been translated into French. The second book was published in 1909, later editions in 1912, 1914, and one in 1916. The title of this book is "Wireless Telegraphy." It has been translated into French and into English.

I further published about thirty papers containing the results of my research work in radio telegraphy. These papers refer to different objects in radio telegraphy, such as the improvement of the Braun tube and the develop-
10542 ment of methods in its use for radio telegraphic measurements.

I had published the method used by Dr. Chaffee into magnetic developing the beam of the Braun tube in 1902, and in 1904 I have used this method for giving in its exact experimental proof for the linear decrease of the amplitude in condenser circuits containing a spark gap, to which reference has been made during the last days. I further made investigations into the theory and practice of coupled circuits; into the theory of dielectric antenna; into the theory of the propagation of the electro magnetic

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waves in wireless telegraphy; into the behavior of the arc generator, and the theory of the so-called frequency changes.

In addition to that, a number of my students, under my supervision, have done research work in wireless telegraphy. The paper of one of them, Taege, having been cited in the report of the Bureau of Standards.

Q. In this case? A. In this case. That is all.

(Whereupon further proceedings are adjourned until Monday, July 24, 1916, at the hour of 2 o'clock 10544
p. m.)

July 24, 1916, 2 o'clock p. m. Continuation of proceedings pursuant to adjournment. All parties present as at former hearings.

Mr. Skeel: If the court please, pursuant to the order of the court, certain tests of defendant's receiver were made in the presence of the assessors, and I should like to have the assessors report now upon those tests, describing them and giving the tabulated results.

Mr. Betts: If the court please, before the 10545
assessors make their report, I would like to ask either Mr. Marriott or Mr. Magnusson two or three preliminary questions, in order that I may lay the basis for an objection on my part.

Assessor's Preliminary Report of Defendant's Surrebuttal Tests of Defendant's Receivers.

R. M. Marriott, one of the Assessors, takes the stand and is examined and testifies as follows:

Q. (Mr. Betts.) Mr. Marriott, it is a fact, is it not, that in all of the tests made or conducted by the plaintiff before you and Mr. Magnusson on April 15th and 16th, at the L. C. Smith Building, these tests were made on the defendant's standard receiver, such as illustrated by the receiver now before the court indicated as B, where the coupling between the secondary and primary coils was made by a movement of an arm so as to bring the secondary adjacent to the primary in its tightest position? A. As I understand the question, that was the case.

Q. (Mr. Betts.) And during these tests it is also a fact, is it not, Mr. Marriott, that the tests were only made on
10547 that receiver by shunting or not shunting a condenser across the secondary? A. To answer that question I would have to look at my notes taken at that time.

Q. Please do so. A. This is April the 15th and 16th you are referring to?

Q. Yes. A. (Referring to notes.) There is some error in the date. The 16th was on Sunday. On the 15th—what occurred on the 15th would be in answer to your question.

Q. Then I want to correct my previous question so as to read the 15th and 17th. A. The 15th and 17th are the dates you refer to, I presume.

10548 Q. Yes. A. I have not examined each detail of those tests, but as near as I can see, the tests were made with or without the condenser across the secondary.

Q. How many tests were made on the——

Mr. Hughes: (Interrupting.) You mean "with and without" instead of "with or without."

A. The tests were first made with the condenser across the secondary and, second, made without the condenser across the secondary—or vice versa.

Q. (Mr. Betts.) How many tests were made of the

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defendant's standard receiver during those tests of April 15th and 17th, at the request of Mr. Thompson?

(The witness examines notes.)

A. I find that five tests were made at the request of Mr. Thompson.

Q. Have you a memorandum as to any tests which Mr. Thompson asked you to make at that time, on April 15th and 17th, and which were not made? A. I have such a memorandum.

Q. Will you just state how many tests Mr. Thompson 10550
asked to have made during the tests of April 15th and 17th? A. If this answer will suffice, I will say at the beginning of the tests on the galvanometer on the 17th, I think it was, Mr. Thompson made a request that his former requests be duplicated in this galvanometer series; the former tests being made with the telephone receiver, and Mr. Thompson made some other remarks to that effect as the tests progressed.

Q. On Saturday last, and Monday, today, did Mr. Thompson ask you to make any of these tests, or did he offer to make any of these tests which he said he wished made on April 17th? A. You mean, did he ask 10551
me to make any of these tests which were made on Saturday?

Q. No; did Mr. Thompson, during Saturday and today, either make these tests which he asked to have made on April 17th, or offer to make them, using the same apparatus that was used on the 17th of April? A. That would require several references to these notes to tell whether they were identical or not.

Q. Well, I wish you would refer—you understand, Mr. Marriott, I am referring to a standard receiver with and without a condenser shunted across? A. I think

I can answer that without reference to the notes; that where the condenser was used across the secondary in the tests of Saturday, that the secondary known as J1 and J2, which is not constructed as in the standard receiver—was used.

Q. Then any tests that were made on Saturday and Monday were not made with the condenser shunted across the secondary as in the standard receiver? A. As I understand your question, they were not.

Q. Were any tests made on Saturday or Monday solely and only on defendant's standard receiver, that
10553 is having only the means provided for varying the coupling as illustrated in this receiver B with respect, first to the condition when the condenser was not used across the secondary, and the condition when the condenser was shunted across the secondary? A. No.

Mr. Betts: That is all the questions I have; do you wish to ask him any, Mr. Skeel.

Mr. Skeel: I wish to have the assessors make their report; to elicit the full information it will be necessary to have the assessors state the full facts of these tests and the conditions under which they were made. I have no objection if Mr. Betts
10554 states his objections to the tests and I can answer them by stating exactly what was done, if you wish, Mr. Betts.

Plaintiff's Objections to Report of Tests.

Mr. Betts: Now, if the court please, I wish to object to the tests conducted Saturday and today, tests lasting up to 12:30 o'clock Saturday night and until 12 noon today, on the ground that the tests were with respect to an apparatus, a receiver of the defendant, not at issue in the case.

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Second; on the ground that the tests before the assessors on Saturday and Monday were of a receiver which was not referred to in the prima facie case of the plaintiff and

Third; on the ground that it was with respect to a receiver not referred to in the rebuttal case of the defendant; and, furthermore, not referred to in the defense's own answering proofs, and, hence, that the tests are wholly immaterial to any of these issues, and it is not proper surrebutttal testimony. And I make this objection, if the court
please, because I did not understand, and I am 10556
quite sure that the court did not understand, that the tests which were actually conducted were to be conducted on anything except the standard defendant's receiver, such as represented in the receiver before you; and the statement was made by Mr. Skeel that he wanted to check up, as he put it, our tests of April 15th and 17th, because Mr. Weagant had promised at that time that certain tests which Mr. Thompson asked to be conducted and had not been conducted, would be conducted. Now, you have heard Mr. Marriott say that on Saturday and Monday Mr. Thompson
did not ask to have the tests conducted which he 10557
had previously asked to be conducted, and that none were made, so that I was under a misapprehension, and I am sure the court was, because that statement clearly appears in the record, on page 206, by Mr. Skeel's statement.

Now, if the court please, the defendant's tests on Saturday and Monday were on the receiver which is before you and marked A. If your honor will note, the coupling, or the degree of association between the yellow coil and the green coil in the standard receiver is adjusted by swinging the yellow coil to and away from the end of the green coil or the primary coil——

The Court: I can see it.

Mr. Betts: (Continuing.)—by the movement of this arm. Now, the receiver which they constructed took out this arm entirely—it is not there —and instead of a secondary yellow coil being so adjusted as to move up and adjacent to the end of the primary coil, they have now got an arrangement whereby this secondary coil can be wholly
10559 inserted inside of the primary coil; the purpose of that being to secure a tighter and a closer association betyeen the yellow coil and the green coil than was possible to secure in their standard receiver which we brought this suit against, which we referred to in our prima facie case, which the defendants talked about and which all of our tests, as Mr. Marriott said, were made on in April the 15th and 17th. Hence this receiver is practically a new receiver interjected into this case in surrebuttal, or attempted surrebuttal, without any proof whatsoever in the first place that these defendants have ever manufactured, either before or
10560 since the filing of the bill, a receiver in which the secondary coil could be moved in and out of the primary coil.

As I said a moment ago, I understood, and I am sure the court understood, and I think Mr. Skeel understood, from his statement in the record, that the tests which were to be conducted by Mr. Thompson were solely upon the receiver which we tested at the Smith Building on the 17th of April, but instead of that, Mr. Thompson has devised this other arrangement, and more than that, they have produced a second coil for the purpose of inserting it in this receiver and conducting certain tests with that on Saturday and today. They took off a number of the turns of this coil—seven or eight or ten, whatever the number may be—so that today's tests were different even from Saturday's tests, with a different coil today from what they had on Saturday.

Now, if the court please, I am making this objection because I say that it is not surrebuttal; that it is not an issue in this case, and if we are going to have different receivers put in at this 10562
stage of the case, why, naturally, of course, the plaintiff will have to have an opportunity to reply, even if your honor should receive these reports, and any testimony over our objection that is not responsive either to the rebuttal or the prima facie case, or within the issues of the suit, and, therefore, for that reason I raise that preliminary objection.

Mr. Skeel: Now, I wish to ask Mr. Marriott a question or two.

The Court: Proceed.

CONTINUATION OF PRELIMINARY EVIDENCE OF ASSESSORS. 10563

Q. (Mr. Skeel.) Mr. Marriott, have you your notes there on the tests made at the L. C. Smith Building, by the plaintiff? A. Yes.

Q. Will you look and see whether at the conclusion of test No. 59, Mr. Thompson requested a test, and what notes you have there with reference to that?

Mr. Hughes: What page of the record are you referring to?

10564 Continuation of Preliminary Evidence of Assessors.

A. (By the Witness.) I have, in connection with the end of test No. 59, this note: "One of Mr. Thompson's tests came in here (referring to his test No. 35, in the former series), and Mr. Thompson requested it, but plaintiff said they would not make the test now, but it could be gone back to later."

Q. Now, will you look at the notes made at the conclusion of test No. 64? A. "43 and 44 tests of Thompson's came in here—postponed."

In that I refer to this test 64, or some tests in between 64 and 65 corresponding to tests which had been
10565 requested by Mr. Thompson and were numbered 43 and 44 previously.

Q. Now, will you look and see what notes you have with reference to any request made by Mr. Thompson at the conclusion of plaintiff's receiver tests? A. After test No. 72, on the next page I have made a note which says: "Thompson made general request for tests, but Weagant said did not want to stop tests; he could have opportunity after Weagant was through."

I think that note there was made after Thompson again called my attention to the fact that he made this request, and wanted to know if I had noted it and I said I remember it and would note it then—I think that was
10566 done some hours after the tests were made.

Q. And were those tests requested by Mr. Thompson ever made? A. No.

Q. Now, Mr. Marriott, referring to the two receivers which you see before you, I will ask you to state whether or not—these receivers have numbers, have they not? A. These receivers have numbers on the wooden box here that fits on them. That number is——

Q. Do you recognize this receiver—is there any mark on it anywhere to identify it by? A. Yes. This is marked by me with the letter "A," and the other by the letter "B."

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Q. You recognize this receiver marked by you with the letter "A" as defendant's standard receiver?

Mr. Betts: I object to the question, because it asks the witness to testify as to the equivalency between the two receivers, and he has not done so.

The Court: Let us see what the answer is.

Mr. Betts: He has not been called as a witness.

The Court: I want to find out what his answer is.

A. (By the Witness.) I recognize that as a defendant's standard receiver, with the exception that the 10568
secondary coil is one which I do not believe has heretofore come under the name of the secondary coil of the standard receiver.

Q. (Mr. Skeel.) Now, the coil that is now in this receiver as placed there, will you please inform me which coil that is, according to your letters or marks? A. That is "2J."

Mr. Hughes: Where is the box?

A. The other box—Mr. Thompson was to get it.

Mr. Thompson: Yes, and I neglected to bring 10569
the top of the box.

Q. (Mr. Skeel.) With this receiver A, in the first series of tests at the University which coil was used? A. 1J, which you have in your hand.

Q. Will you please state how the length of that wire and the size of the wire compares with the length and size of the wire of the coil in defendant's standard receiver?

10570 Continuation of Preliminary Evidence of Assessors.

Mr. Betts: Which do you mean as the standard receiver?

Mr. Skeel: Defendant's standard receiver.

Mr. Betts: What standard receiver?

Mr. Skeel: Well, they only had one standard receiver, the one to which the witness has been testifying.

Q. Does that appear on your notes? A. Yes. I can explain it now or bring it in later.

Q. Explain it now. A. We made a comparison be-
10571 tween the dimensions of the secondary coil similar to the secondary coil which I am inserting in now in this receiver B. We made a comparison with the dimensions of that secondary coil with the dimensions of this secondary coil which I hold in my hand and which is marked "1J," and from those dimensions, counting the number of turns of wire and considering the two layers in the case of the secondary in receiver B, I estimated that there is approximately the same length of wire in the two secondaries.

Q. In the two secondaries of the two receivers A and B, is that correct?

10572 Mr. Betts: No.

Mr. Skeel: Using coil J1 in receiver A?

A. Using coil J1 in receiver A, the secondary then has, approximately, the same length of wire as the secondary used in Exhibit B.

Q. Now, state to the court whether or not there is any difference between those two receivers, other than that this coil J1 being of the same length of wire and the same size of wire, is permitted to be so adjusted in receiver A, as to permit the coupling in any case to be made as close or as loose as desired?

 10573

Mr. Betts: I do not know whether Mr. Marriott has critically examined these two receivers so as to be able to say. A. I believe there is no other difference that is pertinent to the case—there may be some small detail differences.

Mr. Skeel: Now, I wish to make a statement to the court.

Mr. Betts: I would like to ask a question, if you have finished.

Mr. Skeel: All right, I have.

Q. (Mr. Betts.) Mr. Marriott, comparing the coupling 10574
arrangements on receiver A, the coil J1 used with the coupling arrangement of defendant's standard receiver B; is it not a fact that in the former you can adjust the degree of coupling at least twice as tight.

Mr. Skeel: That is true, if the court please, and Mr. Marriott does not need to take the time to study that out. That is what I stated the other day in my request for these tests.

The Court: Proceed; he has stated that as a fact.

Mr. Betts: That is true?

Mr. Skeel: Yes. 10575

Q. (Mr. Betts.) Would there not be from that thing, Mr. Marriott, a very different operation between the two receivers? A. Apparently there is a different operation between the two receivers.

The Court: How is that?

The Witness: There apparently is a different operation between the two receivers.

Mr. Skeel: That is also admitted, if the court please, and that it is exactly that difference that these tests were proposed for.

The Court: Any further questions?

Mr. Betts: No, your honor.

Statements of Counsel In Re Objections to Assessor's Report.

Mr. Skeel: Now, if the court please, I do not wish to take the time of the court with undue defenses of my own conduct in this court room, but I wish the court at its leisure to read the rec-
10577 ord at page 3207, (Print, p. 3357) where I made the request for the tests, and where I fully and exactly apprised the court and Mr. Betts, of exactly what those tests were, in, I think, about the same words that I shall now state what these tests and their purpose was; and that appears on page 3207 of the record. There was not the slightest equivocation of any kind or character; there was not the least attempt to mislead Mr. Betts or the court or anyone else. Now, if the court please, in order to understand the materiality of this and why the plaintiff objects it is necessary to go back into
10578 the history of this case.

The Court: Make it as brief as possible.

Mr. Skeel: I will; it will not take to exceed five minutes.

In plaintiff's prima facie case there was no attempt to make any tests of the receiver; simply general statements were made to the effect that the receiver was in general structure similar to the receiver of the Marconi patent in suit, and, therefore, an infringement. In the course of the defendant's case a number of tests were made,

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the purpose of which was to show that the defendant's receiver was not tuned; that is, that the time period of the secondary circuit could not be varied and was not varied. In rebuttal—now, if the court please, I want this understood that this matter came up for the first time in rebuttal, without any attempt to lay the proper foundation, over my objection, as the court will recall—plaintiff then proposed and carried through a series of tests in the presence of the assessors, by which they took the defendant's standard receiver and 10580
they had a switch operating so that it could be made at one instant the standard receiver of the defendant, and at the other instant it could be made an entirely different receiver; that is with the addition of certain condensers—three different condensers across the secondary coil of that receiver.

Now, the purpose of those tests, as stated by counsel, was for the purpose of showing, first, that the defendant's receiver was broadly tuned to a range of wave lengths between three and six hundred and, second, to show that if the defendant's receiver were transformed into a tunable receiver 10581
by including a variable condenser in the secondary, that it would be more efficient, and so the plaintiff compared the defendant's standard receiver with itself as transformed by the addition of a condenser, and they found that at a certain range of wave lengths, beginning I believe at seven or eight hundred, that the receiver with the condensers added, thereby making it tunable, showed a gradually increasing efficiency, that is louder signals, and the plaintiffs came into court

and told your honor that the reason for that was that whatever utility the defendant's receiver did possess it had it by virtue of the fact that it was broadly tuned to a range of wave lengths of between three and six hundred.

Now, if the court please, the plaintiff totally neglected to inform the court that when the range of wave lengths got over a certain amount that the coupling became loose automatically; simply by virtue of the construction of the receiver the
10583 coupling become loose and the result of that would automatically make the intensity of the signals decrease over that wave length. There is testimony, therefore, before this court to this effect, that by adding the condensers they got more intense signals on the higher wave lengths and the plaintiffs have testified that that is because this condenser is not tuned at these wave lengths, but at the lower wave lengths. This is not the fact. The action noted by the defendant was not a matter of tuning at all. It was a matter of coupling; and that is what these tests of the defendant which we conducted at the University of Wash-
10584 ington were designed to prove, and they have proved it conclusively, which is the reason the plaintiff is objecting to it.

Now, you remember hearing Mr. Marriott testify that the receiver A is, in substance, a receiver of the defendant and has the same size and length of wire in the secondary, and that the only difference is that the impediment is removed, so that it permits the coupling to be made at all wave lengths as close or loose as desired.

Now, the receiver is the same receiver as de-

 10585

fendant's for that reason. Now, then, with that adjustment we conducted at the University of Washington, the same series of tests conducted by the plaintiff. Here is the result, that instead of being weaker on the long wave lengths it is stronger on the long wave lengths and thereby proving conclusively that the action noted by the plaintiff in its tests was due, not to wave lengths, but to the degree of the tightness or looseness of the coupling. If I do not make that clear, I might
say that the defendant's standard receiver with- 10586
out the condenser at the University of Washington was stronger and more intense at the higher wave lengths than the defendant's receiver with the condenser. Thus proving conclusively that the action noted by the assessors and by the plaintiff in the previous tests was a matter of coupling and not of tuning. Now, the tests were exactly the same and the defendant's receiver is exactly the same. The only change made was not a change similar to the one which the plaintiff made in its tests at all. The plaintiff put condensers on and transformed the receiver from an untuned re-
ceiver to a tunable one. All we do was to permit 10587
what was done at the higher wave lengths to be done at the lower wave lengths, that is to permit the degree of coupling to be made where it will produce in both cases—both where the receiver is tuned and untuned, permitted to obtain the maximum result.

I do not wonder that plaintiff objects to the tests in view of the results disclosed—the result has disclosed conclusively that this is a matter of coupling and not tuning; and it has shown also—

10588 Statements of Counsel In Re Objections to Assessor's Report.

so that I may not have to speak again—you will recall that Mr. Weagant made the statement that defendant's secondary coil was intentionally constructed with the length of between three and six hundred meters—approximately, 350 meters, as I recall—so that it could be broadly tuned to the wave lengths between those two ranges; and he went on to say that if that coil were so constructed as to be of a wave length of 200 wave meters or less, it would be absolutely commercially in-
10589 operable.

So, to check up that statement we constructed a coil which when connected with the apparatus has less than 200 meters wave length, that is, 185 meters, and by actual tests, that is by duplicating the plaintiff's tests, we have shown that it is not only not commercially inoperable, but that it is more efficient than is defendant's receiver where the time period of the secondary is between three and six hundred meters, and conclusively proved that it could not be broadly tuned to that range of wave lengths.

Now, I think I have made my position clear,
10590 and I would like the court to check me up and see if I did not use the same language the other day.

I also wish to make the statement that at the time these tests were being conducted and until the results were known, Mr. Betts made no objection to the tests as they were going on.

Mr. Betts: If the court please, naturally I made no objection to the tests as they were going on, because we were not before the court—we were at the University of Washington until 12:30 o'clock on Saturday night and were there until

 10591

12 o'clock this morning—but at the first opportunity I have made the objection.

The real fact, and the question at issue, if the court please, in this case is; "Is the defendant's receiver such as they constructed and put upon the market prior to the filing of the bill, and represented here as receiver B, with the means that that receiver had in it for coupling or adjusting the secondary and the primary—is that receiver an infringement or not an infringement of this patent"? 10592

The defendant, they say, have the limitation in the construction of their device, but that device was constructed by them with that limitation, if it be a limitation, on it, and that is the receiver they put upon the market; that is the receiver, and the only receiver which we referred to in our opening case, which the defendants referred to, or which we referred to in the rebuttal case. But now, if the court please, instead of that they come along and construct another receiver. In that, as Mr. Skeel has said, they take out that limitation, so that now they can get, as they have admitted, at least twice the tightness of coupling between the secondary and the primary as was in 10593 their standard receiver, and as Mr. Marriott told you, the action of these two receivers, is, therefore, different.

Now, if the court please, I think I have stated the position exactly and clearly. This was the only receiver that was in the case until Saturday. It was the receiver which Mr. Kolster tested himself at the Bureau of Standards; it was the receiver which Mr. Kolster tested at the Kilbourne

& Clark laboratory; it was the receiver which was tested before you, if the court please, in the radio room downstairs; and it was the receiver, and the only receiver which was tested by the plaintiff and, I therefore, object to any other receiver, with other structural arrangements bringing about different results and modes of operation, to be interjected in this case in surrebuttal.

Mr. Hughes: May I call the attention of the court to one matter? Mr. Skeel has referred to
10595 what Mr. Weagant testified to as affording one reason why the evidence is admissible. Mr. Weagant's testimony referred to, is as follows:

> "Q. If the defendant's receiver had its secondary so constructed as to have a natural period of say 200 meters, or less, how would defendant's receiver operate, in your judgment, to receive wave lengths of 600 meters?
>
> "A. It would operate very poorly, so poorly that I don't think anyone would ever think of using it for commercial purposes"——

and so on. In other words, if this secondary coil had a sufficiently less number of wires
10596 upon it and a natural wave length of 200 meters or less, it would be inoperative, or "inoperable" as Mr. Skeel says—that is, referring to this receiver which is the matter in issue in this controversy and simply involves the question, to put it another way, whether if this secondary coil had less wiring on it so that the natural wave length would be 200 meters or less, would it be commercially operatable on the wave length of 600 meters, that is, when receiving wave lengths of 600 meters, and it does not involve the

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question of whether a different kind of association of coils—a differently constructed receiver might be efficient or not efficient. It applies to the receiver that is the subject of this controversy, and that receiver as constructed and used by the defendant; and there is one suggestion I want to make only, and that is this: If the court should be of the opinion that the new receiver offered here—I am assuming the possibility that the court might be of the opinion that the receiver constructed so arranged as to be operated and used 10598
as the new receiver over here, were the one, or one of two receivers, we will say, in issue in this case, and B were the other one of the two receivers, and the court were of the opinion that this receiver being broadly tuned is an infringement. In other words, that it is not a matter of exact tuning, but of relative tuning, and that constituted infringement; and that the other is not so tuned as to constitute infringement; you have the precise question here involved in this particular case; this receiver is the only one in controversy. The one now offered is not at all in controversy. Hence although in the case I have illustrated, the court 10599
might find one receiver to be an infringement and the other not to be an infringement; to receive evidence in respect to a receiver which is not at all in issue, and to determine whether it is true that that receiver is an infringement on the plaintiff's receiver, is wholly immaterial to this controversy.

Mr. Skeel: I agree absolutely with Mr. Hughes that this receiver is the only one in issue. There is no question about that; the receiver with the

condensers across it is not in issue, and the receiver with the permission of making closer coupling is not in issue. But the plaintiff took this receiver that is in issue and changed it by putting condensers across the coils, thereby permitting it to have the benefit of closer coupling than the other. So then, what we have done is simply to prove by this simple experiment, not something about this receiver, but to show that the questions that the plaintiff put about this receiver
10601 were wrong, and that the plaintiff's attitude about this receiver was wrong, and that is that, instead of being due to tuning, these results were due to coupling. Of course, any other receiver than this is not in issue or in controversy.

Mr. Betts: I wish to say one word. I want your honor to understand that we have entered these objections because we think it is proper. I cannot let pass the remark of my friend, Mr. Skeel, that they have proved that this receiver is an untuned receiver; and if the court, notwithstanding our objections, receives this testimony, then we shall ask the court for time in which to conduct
10602 reply tests upon this new receiver, and time of the court in which to put in our testimony in respect thereto; and I am informed by our engineers that we can show exactly the same results with respect to the new receiver as we have shown in respect to the old one. I make that statement now so that the court may not be under any misapprehension.

 10603

Rulings of Court on Objections to Assessor's Report of Defendant's Surrebutal Tests.

The Court: I am constrained to the conclusion that this testimony should be received. I do not know what weight it should receive in the consideration of all the testimony which has been presented. Of course, this receiver, Exhibit B, is not in issue. The issue is with relation to the receiver marked Exhibit A, I believe.

Mr. Betts: It is the other way around, your 10604
honor.

The Court: Very well, then, the receiver marked B, is the receiver in issue, and the receiver marked A, is not in issue; but just what weight the experiments with this receiver should have in relation to the entire testimony and the consideration of the entire testimony, I am not at this time able to state. So I think the testimony should be received and considered finally when it is finally disposed of; proceed—the objection is overruled.

Mr. Betts: In view of your honor's ruling, we shall have to ask, and we do now ask leave of
court to conduct reply tests, and for an oppor- 10605
tunity to take the testimony with respect thereto, and I will have to ask your honor to modify the arrangement which you have made for the conclusion of this trial and hearing.

Mr. Skeel: If the court please, I suggest that that subject of discussion be reserved until after the assessors' report is received.

The Court: I do not understand that you desire to press this suggestion now, but it was simply rather as a notice to the court that you desired to make such an application.

Mr. Betts: Certainly, I would like to give notice to the other side, and advise the court that we shall need an opportunity to make replying tests, and to introduce testimony after our tests have been conducted.

The Court: Well, we will take that up when the necessity therefor has been shown in court.

Mr. Betts: I beg pardon?

10607 The Court: I say, we will dispose of that when the necessity for such further tests has been demonstrated in court.

Mr. Betts: Well, we are quite convinced, your honor, on that point, and I was moving now——

The Court: Very well, I will dispose of it later.

Mr. Betts: I presume that we may have an exception to your honor's ruling in regard to the introduction of the assessors' report or any testimony which may be offered with respect to the report.

The Court: Just note in the record that this testimony is received over the objection of the
10608 plaintiff and an exception is noted. Proceed.

Mr. Hughes: And if the arguments have been taken down they will show the grounds of the objection and the exception, otherwise we might state the grounds.

The Court: It has all been taken down, so that it will all speak for itself.

Assessor Marriott's Report of Defendant's Surrebuttal Tests of Defendant's Receivers.

Q. (Mr. Skeel.) Will you, very briefly, describe the tests conducted at the University of Washington on July

23 and 25, 1916, in the presence of the assessors? A. The assessors' report relates to tests made by the defendant July 22 and 24, and includes tests Nos. 80 to 105, inclusive.

The first group of tests, 80 to 89, inclusive, show some comparative results obtained when a certain receiver secondary was used alternately with and without a condenser across the secondary.

The second group of tests, 90 to 96, inclusive, show some comparative results obtained from two differently constructed receiver secondaries. 10610

The third group, 97 to 103, inclusive, were for the purpose of ascertaining the fundamental wave lengths of secondaries used in tests 80 to 96 inclusive.

The fourth group of tests, tests 104 and 105, show some comparative results obtained from two differently constructed receiver secondaries.

Work was begun on these tests about 9:30 a. m., July 22, 1916, at the place of business of the Kilbourne & Clark Manufacturing Company, 81 Columbia Street. The Marconi Company representatives, Betts, Waterman and Weagant; the Kilbourne & Clark representatives, Skeel, Farnsworth and Thompson and the court assessors Magnusson and Marriott were present. 10611

Mr. Thompson made a preliminary statement to the effect that he would use a receiver like the one used in the L. C. Smith Building tests, except the secondary is longer, but contains the same size wire wound in one layer, where former secondaries contained two layers, and that this new coil would slide inside of the primary coil and that this new coil has the same length of wire as the secondary in the standard receiver.

The receiver offered was then examined and the following was noted. The case, or wooden box used to cover

10612 Assessor Marriott's Report of Defendant's Surrebuttal Tests of Defendant's Receivers.

this receiver, had on it a name plate which was marked "Type C, Form 4, No. $\frac{K.M.}{611}$ serial No. 1041, Kilbourne & Clark Manufacturing Company."

A secondary coil as used in the standard receiver was offered for measurement and was found to have an outside diameter of 4¼ inches and a length of 2⅛ inches, wound in two layers, 49 turns per layer.

A new secondary coil, which was marked 1J, was offered and measured and found to have a length of 5⅞ inches, and a diameter of 2-15/16 inches, with 133 turns.

10613 My notation is that these dimensions indicated about the same length of wire on the two secondaries.

With the new secondary as arranged when the coil ends are just opposite the indicating bar reads 3¾, and when the indicating bar reads zero the secondary coil is about 2⅛ inches out of the primary coil, that is that its nearest end is 2¼ inches, from the nearest end of the primary; and when the secondary coil is all inside of the primary coil the scale reads about 10. The scale from which these readings were made is on the handle used to pull the secondary coil into the primary. For the purpose of the tests the buzzer excited Kolster decremeter was used. This decremeter was marked "Type C, Serial

10614 No. 109," and is an instrument having a range, according to the markings, of from about 150 meters to about 4,700 meters wave length. The dummy antenna used and connected to the receiver and coupled to the Kolster decremeter, consists of two Leyden jars in series, with the resistance wire set said to have a resistance of about six ohms. This resistance wire and the Leyden jars were in series with the coupling coil of the decremeter and the primary of the receiver under test. An additional coil was used in series with this circuit, consisting of eight turns of wire. Condensers to be used for connecting

 10615

across the secondary were offered, which condensers appeared to be the same or similar condensers to those used at the L. C. Smith Building tests, and in these tests have been marked as in the L. C. Smith Building tests, that is, "C_1, C_2, and C_3."

Mr. Thompson made a further statement, to the effect that the tests will be run by placing the same coupling limitations on the old coil, not making the coupling closer than four and a half, but it can be made looser than that; in which case the coupling is to be adjusted for best signal, so long as no closer coupling than four and a 10616
half is used.

Mr. Hughes: Four and a half per cent.?

A. (Continuing.) Four and a half is the scale reading on the handle which is used to move the secondary in and out of the primary.

Unsuccessful attempts were made to carry on the tests up until about 12 o'clock; when the tests were temporarily discontinued because of the instability of the galvanometer, and the data taken was, by common consent, cancelled—the tests were discontinued.

Q. (Mr. Skeel.) Just a moment; the galvanometer is 10617
a measuring instrument? A. The galvanometer is a measuring instrument which was offered here to measure the comparative working of the various uses of the receiver.

The tests were continued at the engineering building of the University of Washington at 2 p. m. The stated intention being to follow much the same scheme as that the plaintiff's L. C. Smith Building tests, made in April; adjusting coupling to best advantage, first without and then with a condenser across the secondary, the condensers being condensers C_1 and C_2 and C_3. Briefly, 10618 Assessor Marriott's Report of Defendant's Surrebuttal Tests of Defendant's Receivers.

it was understood group 1 tests, 80 to 89, inclusive, were for the purpose of ascertaining whether or not a Kilbourne & Clark receiver, provided with an altered form of secondary, worked better with or without a condenser across the secondary; greater deflection of the galvanometer, being offered to indicate better working of the receiver.

The comparisons were made on nine different wave lengths, ranging from 300 to 3825 meters. As a whole, the galvanometer indicated equally good or better work-
10619 ing without the condenser across the secondary. In the detail report which will be offered, two exceptions will be found to this general statement; namely, Dr. Magnusson's reading of the galvanometer in test 88, and my reading in test 84.

Mr. Betts: At that point, will you indicate in what way the secondary was altered, as you have just mentioned.

A. The secondary which I have referred to as the altered secondary is one which consists of a cylinder wound with one layer of wire and of such dimensions as
10620 to make it capable of being moved in and out of the primary.

The Court: Just identify it in the record—is that the one which you testified about before?

The Witness: This is the secondary, marked 1j.

Mr. Hughes: The primary consisted of a wire wound around a hollow cylinder, so that the other may be inserted inside of that cylinder, is that what you mean?

 10621

A. Yes. The primary consisted of wire wound around a hollow cylinder, and which is shown, or which appears in Exhibit A, at the lower left hand corner as I face the back of the instrument.

The secondary of the standard receiver differs from this altered secondary, in that its diameter is too great to enter the inside of the primary, and not great enough to slide over the primary; and in that it consists of two layers of wire instead of one layer of wire, as in the altered secondary, marked 1j and, as I said before, according to the measurements I made, I believe the length 10622
of wire is the same in the two secondaries.

> Mr. Hughes: Mr. Marriott, don't you think you should also describe the difference in the manner of association of the primary and secondary; you have described the manner of associating the secondary in the new apparatus used, for the purpose of the record I think it would be well to state the manner of associating the secondary with the primary in the original, or in the defendant's regular standard receiver.

A. With the defendant's standard receiver the secondary is arranged to be swung on an arc, 10623
whereby the axes of the secondary may correspond to the axes of the primary, and whereby the secondary may be put in the closest relation to the primary, with the end of the secondary wiring nearly against the end of the primary wiring when the handle which caused the coil to swing on the arc is at the point marked "90" and the coupling may be loosened by swinging the secondary coil on this arc away from the primary coil to a point marked on the coupling handle "zero"; at which point the axis of the secondary is at right angles

to the axis of the primary, that being the point of minimum coupling; and the point of maximum coupling being the one where the axis of the secondary is coincident with the axis of the primary.

Now, in the use of the secondary, marked 1j, this secondary has its axis always coincident with the axis of the primary, but the coupling variation is brought about by sliding the secondary farther away from the primary to obtain a looser coupling and, to obtain tighter coupling, by moving the secondary inside of the primary,
10625 whereby the secondary turns are completely enveloped by the primary turns, and thereby obtaining the maximum coupling by so locating this secondary and primary as to have, approximately, the center of the winding of the primary over the center of the winding of the secondary; which gives a closer coupling than is obtained with the standard receiver with the standard form of secondary.

In the detail report about to be offered in tabular form, it will be seen that references are indicated as "Note, 1, 2, 3," etc. Those notes will be read into the record after offering the tabulated report. The detail report of tests 80 to 90, inclusive, in tabular form, is now
10626 offered for insertion into the record, because I believe it is very much easier to be copied into the record than for me to try to read it into the record.

The Court: Is there any objection?

Mr. Betts: No objection.

The Court: Any objection to having it copied in the record?

Mr. Betts: No objection, of course.

(Whereupon the same is copied into the record as follows):

(TABULATED REPORT—DEFENDANT'S SURREBUTTAL TESTS NOS. 80–89 OF DEFENDANT'S RECEIVER WITH AND WITHOUT CONDENSER.)

Test Number	Kolster Decremeter Degrees	Coil Used with Decremeter	Wave Length Meters	K & C Type C Receiver A Serial Number 1041 Receiver Primary Inductance				Condenser Across Secondary Readings in Div.	Coupling Coil In.	Series Primary Condenser	Galvanometer Readings by Magnusson	Galvanometer Readings by Marriott	References
				1	2	3	4						
80	134.5°	4	3600	11	5	11	5	None	10″	None	10.5	10	
"	"	"	"	11	5	11	5	C_1-40°	5½″	"	8.8	8.	
											13.9	13.9	Note 1
81	134.5°	4	3600	0	5	11	6	None	10″	"	9.9	9.	Note 2
"	"	"	"	0	6	11	6	C_1-12°	6¼″	"	13.5	13.5	
82	146°	4	3825	11	0	11	11	None	10″	"	11.5	8.	Note 3
"	"	"	"	11	0	11	9	C_1-41°	7″	"	8.3	7.1	
83	116.5°	4	3250	1	0	11	4	C_1-0	10″	"	13.2	14.	Note 4
"	"	"	"	1	0	11	4	C_1-10°	6⅛″	"	7.8	9.	Note 5
"	"	"	"	0	0	11	4	C-0	10″	"	10.5	....	A second maximum with C-20°
"	"	"	"	1	0	11	4	C-20°	6¼″	"	8.2	....	
84	80°	4	2625	6	0	8	11	None	8″	"	9.9	10.	
"	"	"	"	11	0	8	11	C_2-100°	6½″	"	9.9	10.3	
85	34°	4	2000	3	11	11	2	None	8¾″	"	7.1	7.5	
"	"	"	"	3	11	11	2	C_2-38°	5¼″	"	4.4	4.5	
86	71.5°	3	1015	10	9	2	0	None	8″	"	6.0	5.0	
"	"	"	"	6	9	2	0	C_3-100	4½″	"	5.8	4.5	
87	76.5°	2	600	5	6	0	0	None	9½″	"	7.7	7.	
"	"	"	"	7	6	0	0	C_3-59	4½″	"	7.6	5.5	
88	29°	2	450	9	6	0	0	None	10″	113°	7.5	7.2	
"	"	"	"	9	6	0	0	C_3-10°	4½″	109°	7.7	5.5	
89	104°	1	300	10	2	0	0	None	7″	118°	1.8	1.8	
"	"	"	"	11	2	0	0	C_3-8°	5½″	107	1.8	1.8	

10629 10628 10627

The Court: Proceed.

A. (Continuing) In these tests the Assessors made coupling adjustments when the condenser was in or out.

Note No. 1: It was first suggested that the adjustments without the condensers across the secondary be made by Mr. Thompson while with the condensers across the secondary the adjustments were to be made by Mr. Weagant. Assessors then duplicated the settings and reading, changing from one setting to the other quickly as possible. This was carried out in test No. 81, but some
10631 discussion resulted which caused a different method to be employed in test No. 82.

Note No. 2: With leads as left by Mr. Thompson in his adjustment the reading without the condenser was 13.9. Mr. Weagant reversed the connection of the leads, bringing the reading without the condenser down to 9.

Note No. 3: Owing to argument, Mr. Thompson stated that these were his tests and he would make both adjustments which the Assessors would check and read. Mr. Betts then stated, between tests 81 and 82, that the plaintiff would like to have Mr. Weagant make the adjustment with the condenser on, to be checked by the Assessors. Mr. Thompson then said that the defendant declined, be-
10632 cause Mr. Weagant took so much time that they decided Mr. Thompson should make the tests; and Mr. Skeel asserting that if Mr. Thompson's adjustments did not give the maximum, the Assessors are to check for a maximum effect. Mr. Skeel further added that the apparatus will be left available for Mr. Weagant to duplicate the series for such maximum readings as he may be able to obtain.

I checked for maximum by sound and the maximum obtained by Mr. Thompson seemed to be about as good as could be obtained in the time which I thought was a reasonable length of time.

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Note No. 4: Assessors were requested to check for another maximum.

Mr. Skeel: By whom?

The Witness: I haven't it noted; I think it was by Mr. Thompson.

Mr. Magnusson: By Mr. Skeel.

Mr. Skeel: You have it in your notes that way, Professor, have you?

Mr. Magnusson: Yes, "Skeel".

The Witness: I found a setting of 10 degrees
on the condenser gave a little better reading than
a setting of 20 degrees on the condenser provided 10634
by Mr. Thompson; and the reading I found appears in the tabulated results.

Note No. 5: Mr. Thompson at this point notified Mr. Weagant to make adjustments for maximum. Mr. Betts said plaintiff did not wish to adjust on one, or scattered tests, but to either adjust on all or none.

Adjourned at 6:30 P. M., and started again at 8:45 P. M.

The second group of tests, 90 to 96, were started,
using a secondary coil marked 2j at 8:45, and beginning 10635
with test No. 90 the coil 2j was substituted in place of 1j. 2j had a diameter of 2 15/16 inches, a length of winding of five inches, number of turns 96. The turns were separated by about the thickness of one wire with its insulation. Apparently the length of wire on 2j was at that time about 72 per cent of the length of wire on 1j. Another receiver, type C, from 4, drawing No. $\frac{\text{K.M.}}{611}$ series 1043, with secondary of the standard type, was brought in for comparison with the receiver using 2j secondary.

Beginning with test 90 the receiver with secondary

10636

winding 2j, and which receiver in that condition was called receiver A, is compared with a standard Kilbourne & Clark receiver, called receiver B in the tabulation and tests hereafter.

Mr. Thompson stated that "We will use the same silicon-arsenic detector at all times with both receivers. The switches provided served to transfer the telephone receiver connections and the detector connections and the antenna and current connections from one receiver to the other by simply throwing the six-pole switch from left to right.

10637 The second group of tests, 90 to 96, was offered to indicate whether or not the Kilbourne & Clark receiver A provided with a differently shaped secondary with more spacing between turns and less wire, was superior to a Kilbourne & Clark receiver B provided with the type of secondary used in the standard receiver as used at the L. C. Smith Building. The indication of superiority offered here was louder sound in the telephones and greater galvanometer deflections. These methods of indication showed the receiver A to be better than receiver B. A tabulated report of these tests is offered for insertion in the record.

10638 (Whereupon the same is copied into the record as follows:)

(TABULATED REPORT—DEFENDANT'S SURREBUTTAL TESTS NOS. 90-96 OF DEFENDANT'S RECEIVERS A AND B.)

Test Number	Decre-meter Degrees	Coil used with Decre-meter	Wave Length Meter	Receiver Primary Inductance 1	2	3	4	Long Wave Switch	Receiver Used	Coupling	Series Condenser Primary	Galva-nometer Reading by Mag-nusson	Galva-nometer Read-ing by Marriott	Compar-ative Intensity of Sound in Tel. Receiver
90	135.5°	4	3625	—	—	—	—	—	A	8″	None	11.2	13.5	√
"	"	"	"	—	—	—	—	—	B	90°	"	4.5	5.5	
91	105	4	3050	—	—	—	—	—	A	7¾″	"	17.3	17.5	√
"	"	"	"	—	—	—	—	—	B	90°	"	9.1	9.0	Note 6
92	22	4	1825	8	3	0	0	1	A	6½″	"	8.7	5.3	√
"	"	"	"	11	2	0	0	1	B	90°	"	2.4	1.6	
93	72	3	1015	11	11	1	0	0	A	8″	"	7.1	7.2	Slightly Louder
"	"	"	"	11	11	1	0	0	B	90°	"	6.7	6.8	
94	76	2	600	9	5	0	0	0	A	10″	"	5.8	5.8	Slightly Louder
"	"	"	"	9	5	0	0	0	B	90°	"	4.4	4.4	
95	29	2	450	6	6	0	0	0	A	9″	150°	7.6	7.7	√
"	"	"	"	5	6	0	0	0	B	90°	148°	4.3	4.4	√
96	104	1	300	6	5	0	0	0	A	5″	49°	1.8	1.7	
"	"	"	"	5	5	0	0	0	B	90°	44°	0.7	0.6	Note 7

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10642 The Witness: (Continuing) As in former tabulation, certain notes are referred to on the margin. This is one of the notes.

Note 6: In test 91 Mr. Marriott adjusted the detector to see if he could get better reading than that provided by the setting as made by Mr. Thompson. Mr. Marriott got three poorer readings and one slightly better than that provided by Mr. Thompson.

Note No. 7: Mr. Weagant requested the Assessors to note whether the same wires of the dummy antenna came
10643 to corresponding posts on the two receivers. Note No. 7 is opposite test No. 96. I traced the wires and found that they did connect to corresponding posts on the two receivers.

The next group of tests, that is, the third group of tests, running from test No. 97 to test No. 101, was for the purpose of measuring the fundamental wave-lengths of secondaries used in the preceding tests. In measuring these fundamental wave-lengths in every case Dr. Magnusson and I readjusted the detector used, and each reading given in these is for a different adjustment of the detector.

Test No. 97; Measuring the secondary of receiver b,
10644 which is the standard secondary. I obtained in measuring this the following wave-lengths: 340 meters, 335 meters, 330 meters, 350 meters, 340 meters, 360 meters, the average of which was about 342 meters. Dr. Magnusson obtained the following wave-lengths: 340 meters, 330 meters, 330 meters, 340 meters, 325 meters, 333 meters, his average being 333 meters. In this test No. 97 the detector used was that on the face of the receiver.

Test No. 98; measuring the fundamental wave-lengths of the same receiver as in test No. 97, except to take the detector used in the second group of tests. For this

 10645

fundamental wave-length I obtained the following readings: 450 meters, 465 meters, 460 meters, 450 meters, 435 meters, 445 meters, giving an average of 451 meters. Dr. Magnusson obtained the following readings: 445 meters, 450 meters, 450 meters, 450 meters, 445 meters, 450 meters, giving an average of 448 meters.

Test No. 99; was made to measure the fundamental wave-length of coil 2j, using the detector on the face of receiver A. In this case the smallest coil available with the decremeter was used, and the maximum sound was obtained every time with any setting of the detector when
the condenser pointer came to zero, which indicated that 10646
the fundamental wave-length of this secondary 2j, as used, was either the lowest reading of the decremeter or something below that, in so far as you could tell by sound measurement, within the calibrations of the decremeter, and the lowest calibration on the decremeter is 165 meters, which would mean that that coil 2j probably has a fundamental wave-length of something less than, or in the neighborhood of 165 meters.

Test No. 100; measurement of the fundamental wave-length of coil 2j, used in A, with detector leads and detector No. 333, which was used in the second group of tests, as for example in test No. 96, this test being made
at the request of Mr. Waterman for the plaintiff. The 10647
thing about this test was to see what the fundamental wave-length would be with those leads, and Mr. Waterman requested that those leads be used. For this test I made five settings of the instrument, and obtained the same reading of 285 meters for each one of the five. Dr. Magnusson obtained the following wave-lengths: 285 meters, 290 meters, 290 meters, 285 meters, 285 meters. Dr. Magnusson's average was 287 meters.

Test No. 101. In this test the fundamental wave-length of receiver B was measured with its secondary and leads and detector No. 333, as used on this receiver in the second group of tests, as for example, test No. 96. For this I obtained the following wave-lengths: 490 meters, 485 meters, 600 meters, 605 meters, 600 meters, 500 meters, 515 meters, and 545 meters, the average being 542 meters. Dr. Magnusson took two readings and obtained 545 meters and 535 meters, giving an average of 540.

The adjournment was then made, it being 12:30 A. M., Sunday morning, Mr. Skeel making a statement that Mr. Thompson would make two tests Monday morning at 9:00
10649 o'clock, that is, July 24th, and Mr. Weagant and Mr. Waterman might have the opportunity of checking any tests they might wish at any time Sunday or Monday, excepting between 9 and 10 A. M. Monday morning.

Monday morning, July 24, 1916. Mr. Thompson outlined the purpose of tests he wished to make, in which he said that the tests are to measure the fundamental wave-lengths of the secondaries of the receivers A and B, wherein A used the secondary known as 2j, while B used the regular standard secondary. A switch is arranged to throw from one to the other, using the same detector for each, with leads to detector as short as practicable under the circumstances. Mr. Thompson stated that six
10650 turns had been removed from coil 2j. If such is the case the number of turns would be 90 instead of 96. In this case the galvanometer was connected in series with the telephones instead of in parallel with the telephones, as in former cases. Also, in this case, the particular point brought out was that the leads used to the detector from the two receiver secondaries were much shorter than before, and were not twisted together but were separated an inch or two.

Now, using receiver A the primary inductances were all set at the point marked "11" on the inductance switch on the face of the receiver. The long wave switch was at the position marked "2". The series condenser was set at 180 degrees and the antenna and ground posts marked "A" and "G" on the face of the receiver were connected together by a short length of wire.

The first test was for the fundamental wave-length of altered coil 2j in receiver A, and I obtained the following readings for fundamental wave-length: 180 meters, 177 meters, 182 meters, 180 meters, 181 meters, the average being approximately 180 meters. Dr. Magnusson obtained the following wave-lengths: 190 meters, 181 meters, 180 meters, 187 meters, 180 meters, 182 meters, the 10652
average being 183 meters.

Test No. 103; the same test as No. 102, except the short circuit wire was removed from the posts A and G and the inductance switches, including the long wave-switch, were turned to zero setting. With this change I obtained the following wave-lenghts: 193 meters, 177 meters, 193 meters, 190 meters, and 180 meters. Dr. Magnusson obtained the following wave-lengths: 185 meters, 180 meters, 182 meters, 190 meters, 190 meters, and 181 meters. The average wave-length obtained by me was 186, and by Dr. Magnusson about 185 meters.

Test No. 104; was made under the fourth group and
was made for the comparison of receiver A with receiver 10653
B, where receiver B secondary consisted of the coil 2j having part of the turns removed, or having about 90 turns now. In this test the decremeter was set at 134 degrees, using coil No. 4, which gave a wave-length of about 3600 meters, and the receiver A had its inductances set in the following positions: Inductance switch No. 1 was set at the point marked "11", inductance switch No. 2 was set at the point marked "8", inductance switch No. 3 was set at the point marked "11", and inductance switch No. 4 was set at the point marked "5". The long

wave switch was set at the point marked "2". The receiver used was receiver A, and the coupling was indicated as "10" on the bar which controls the position of the secondary coil. The series condenser used in the primary receiver was out, and with this arrangement the galvanometer deflections, as found by Dr. Magnusson was 2.3, and as found by myself it was 2.4, with the loudest sound for this receiver A. Receiver B had the inductances set as follows: Switch No. 1 was set on butt No. 11, switch No. 2 was set on No. 7, switch No. 3 was set on No. 11, switch No. 4 on No. 5, and the long wave switch on 2.
10655 With this receiver the point of the coupling was indicated as 90, and the series condenser was out, as in the other receiver. With this receiver the galvanometer deflection found by Dr. Magnusson was 1.8, and by myself 1.7, that is, both of us found a greater deflection for receiver A using 2j altered form of secondary than for receiver B, using the standard form of secondary.

In test No. 105 the wave meter was set at 134 degrees, using coil 4, which gives about 3600 meters, and tuner A had its first inductance switch at the point marked "11," the second at zero, the third at "5," the fourth at "11," and the long wave switch at "2." The coupling was indicated as "10" on the rod, and the series condenser was
10656 out. In this case the galvanometer was connected in parallel with the telephone receivers, and the deflection found by Dr. Magnussòn was 22, and by myself 23, the loudest sound being given on receiver A. Receiver B had its inductance switches set; switch No. 1 at "11," switch No. 2 at zero, switch No. 3 at "5," switch No. 4 at "11," and the long wave switch at "2." The coupling was the closest coupling, marked "90" on this instrument. The deflection as found by Dr. Magnusson in this case was 2.5, and by myself 2.5, that is, the greatest deflection was found using receiver A, which employed the secondary

marked 2j, and the loudest sound was found by using the receiver A with the secondary marked 2j.

At this point, following test No. 105, Mr. Waterman requested the Assessors to note how long it took to prepare for this test. My recollection was that it took about thirty minutes to prepare for the test. Mr. Thompson and Mr. Kolster both took part in the preparation. The assessors were then asked to check the settings of this test No. 105, in which the comparative deflections were 22 as against 2.5, and they were tested by the Assessors to a limited extent, as far as they thought desirable in the
time available, and as far as they thought necessary, and 10658
found that the settings given by Mr. Thompson were as good as any that we obtained.

Mr. Weagant then asked the Assessors to note that the leads to the detector from receiver A were in a reverse position in test No. 105, as to the position they were in in test No. 102. Neither of the Assessors were able to recall the position in test No. 102, so we could not state whether they were in reverse position or not.

Mr. Thompson then asked the Assessors to note that if any reversal was made it was made to cause the galvanometer deflection to be in the same direction for each receiver, as if they were not connected, the same the gal-
vanometer would swing one way on one and the other 10659
way on the other.

Mr. Betts then requested the Assessors to note the value of the stopping condenser used in receivers A and B as soon as possible after they were received in court today. Mr. Betts requested the Assessors to take charge of the receivers and bring them to court at 2:00 o'clock p. m. The tests were adjourned at 12:15.

Mr. Betts: Mr. Marriott, have you got any note in regard to whether or not Mr. Thompson offered

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to allow Mr. Weagant to make the adjustments for tests Nos. 104 and 105?

A. I have not any.

Mr. Betts: You have none?

A. No.

Mr. Betts: Wouldn't it be more correct to say that it took Mr. Kolster and Mr. Thompson about thirty minutes to make the adjustments for test
10661 No. 105 rather than to prepare for the tests, as you have phrased it?

A. Well now, to tell the story as near as I can remember it; in test No. 104 Mr. Magnusson in doing something about the apparatus struck the galvanometer with his head, and then Mr. Thompson made some changes in that setting, then Dr. Magnusson made some changes in the galvanometer and Mr. Thompson then when he started to make the arrangements for test No. 105 spoke of the detector being out of order, and about that time I looked at my watch and I think it was then a few minutes after eleven, a very few minutes after eleven, and when the
10662 test was completed it was 11:40, and I noticed during that time that Mr. Thompson was there part of the time, and part of the time Mr. Kolster was there, and they were both there part of the time, and part of the time one of them was there. Whether they were repairing some fault or preparing for the test or just what they were doing, I am not sure.

Mr. Skeel: Mr. Marriott, I want to ask you two questions; can you state how the average coupling compared in tests 80 to 89, inclusive, with reference to when the condenser was on and off—is it a matter of record already, Mr. Marriott?

A. Oh yes, the coupling is given.

Mr. Skeel: Then I withdraw the question. I do not want to take any more time than is necessary. I could not make it out. Next, did the arrangement to permit closer coupling apply both when the condenser was on and off in receiver A?

A. You mean you could get the same manipulation of the coupling? 10664

Mr. Skeel: Yes.

A. Yes, using the condenser did not in any way restrict the manipulation or movement of that secondary coil.

Mr. Betts: All adjustments or settings were made by Mr. Thompson, except as noted in the Assessors' reports?

A. As far as I recall, yes.

Mr. Skeel: And they were all checked for a maximum by the Assessors, is that correct? 10665

A. I think the test No. 104 was not checked for a maximum. I think all the others were.

Mr. Betts: The tests were not checked, the Assessors did not attempt to get the maximum on all of the tests?

A. What the Asessor did was to change the amount of inductance in the primary as it had been left by Mr.

Thompson, and to change the coupling, and where condensers were used, to change the condensers, and in what they considered the amount of time that they should use for the purpose, to see if they could obtain a better maximum, and as I recall it there was one case where I considered I had a better maximum and Dr. Magnusson considered it was not a better maximum, and there was very little difference from the setting I found in that case from what Mr. Thompson had.

Mr. Betts: That does not apply to the last three
10667 tests.

A. The last three tests—test No. 105 we made just such changes, particularly changing these inductances. I do not remember whether we changed the coupling in that test or not.

Witness excused.

DEFENDANT'S SURREBUTTAL EVIDENCE.

DR. J. ZENNECK, called as a witness on behalf of
10668 Defendant, in surrebuttal, testified as follows:

Q. (Mr. Farnsworth.) State briefly, please, of what scientific societies you are a member. A. In this country I am a Fellow of the Institute of Radio Engineers, and a member of the Committee on Standardization of this Institute, and I have worked with the other members of this Committee in preparing its report.

Q. Please give your testimony concerning the plaintiff's Massachusetts tests on July 3 and 4 last of the Simpson mercury valve transmitter, which you witnessed. A. I first checked the connections in the arrange-

 10669

ment of Dr. Chaffee, and as far as I could check them I found that they were correctly represented, or that they are correctly represented in the Chaffee diagram No. 1. I shall, therefore, refer to this diagram.

The arrangement of Dr. Chaffee in the Massachusetts tests consisted of three parts: The feeding circuits, the transmitter, and the measuring arrangement.

A. FEEDING CIRCUITS.

The feeding circuits contain the motor, generator, the reactance, the transformer, the mercury valve, with
the keep alive circuit. 10670

The generator was a 500 cycles, alternating current generator, manufactured for the Marconi Company. I have no experience, neither with this generator nor with that generally used by the Kilbourne & Clark Company for their transmitters. I, therefore, do not know whether one of them was more appropriate for this transmitter than the other. But I do know that the constants of the feeding circuit especially the ratio of the reactance to the capacity to be charged are important for quenched gap transmitters. It seems that in the experiment of Dr. Chaffee there has been some need for adjusting this reactance, as in addition to the field rheostat con- 10671
trolling the voltage of the generator there was used a variable reactance, and its value varied in the course of the tests.

There was in the feeding circuit no power indicating instrument, such as electro-dynamometric, watt meter. I shall come back to this point later on.

B. TRANSMITTER.

(1) SPARK GAPS.

The spark gaps had been opened on the morning of July 4, and it had been found that at least two of them

ments for measuring the energy input and output would have greatly facilitated the decision on the question whether or not the transmitter in the Chaffee tests was operated under the same conditions as in practice.

I was surprised that Dr. Chaffee instead of using a dummy antenna did not make use of one of the actual antennae which were at hand at his laboratory.

(3) THE TRANSMITTER ARRANGEMENT.

The arrangement used in the Massachusetts
10679 tests may be diagrammatically represented by Figure Z-1 (reproduced opposite)—corresponding to Chaffee diagram No. 1. I mark the spark gap G, the small inductance L^1, the condenser C, the spiral S, the lower part of it PQ, the upper part QR, the condenser at the right C^1, corresponding to what is marked in Chaffee diagram No. 1 as "Condenser .001 M. F." The inductance L combines in this diagram [what is] the inductances which are marked in Chaffee diagram No. 1, "L," and variable inductance. As long as the spark gap G is working this arrangement can be described as consisting of a primary circuit, containing the condenser C, the part PQ of the spiral S, the inductance L^1,
10680 and the spark gap, and of a secondary circuit, C^1, inductance L, spiral S, condenser C. But this view is entirely arbitrary, as may be shown by Figure Z-2 (reproduced opposite).

In this Figure Z-2 I marked correspondingly to Figure Z-1 the condenser at the left as C, the spark gap as G, the condenser at the right as C^1, the inductance at the right as L, the spiral as S, the left part of it as PQ, the right part of it as QR, and the small inductance as L^1. In Figure Z-2 the elements and their connection is exactly the same as in Z-1. In looking at this Figure Z-2

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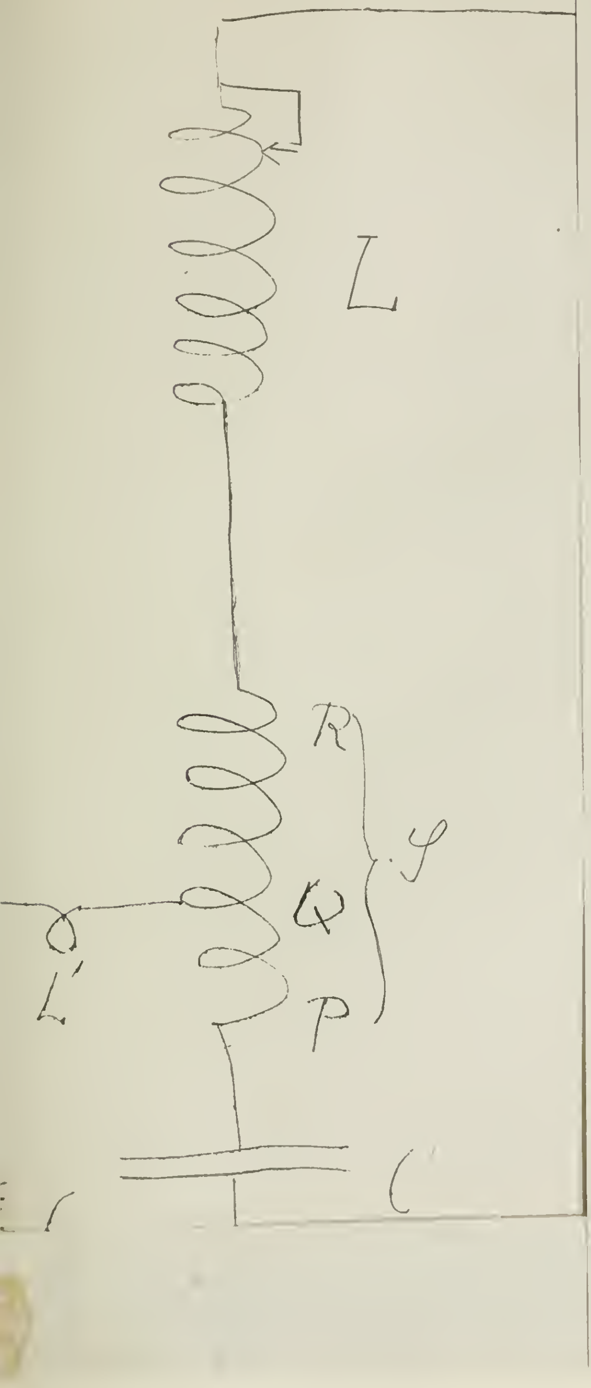

Z 1

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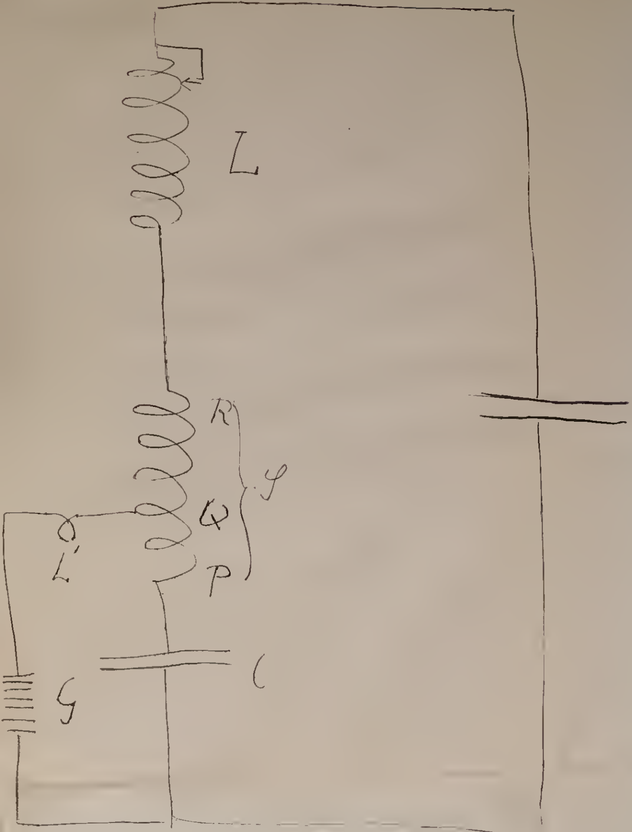

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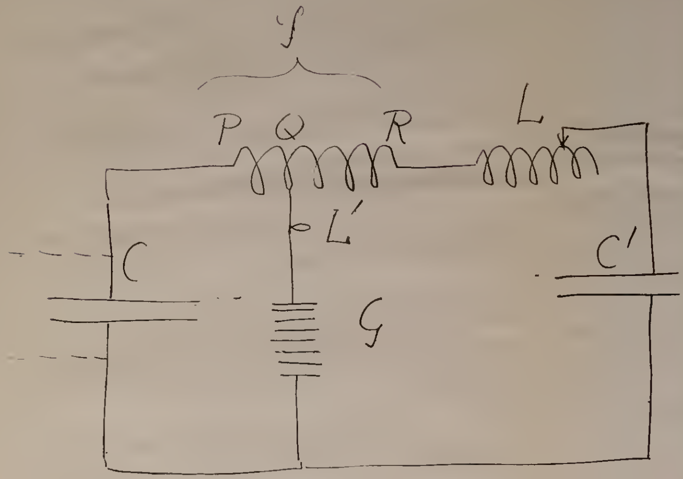
Z 2



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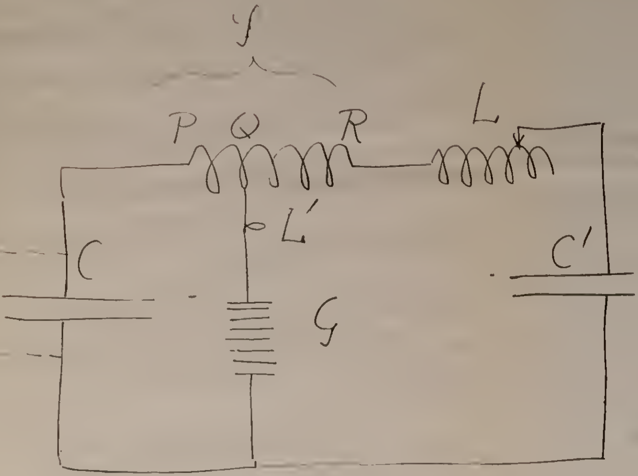
Z 2



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Z 3

I

July 25 16

C

PQ

L′

G.

II

C′

L

RP =

C

10678

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The arrangement used in the Massachusetts
10679 tests may be diagrammatically represented by Figure Z-1 (reproduced opposite)—corresponding to Chaffee diagram No. 1. I mark the spark gap G, the small inductance L^1, the condenser C, the spiral S, the lower part of it PQ, the upper part QR, the condenser at the right C^1, corresponding to what is marked in Chaffee diagram No. 1 as "Condenser .001 M. F." The inductance L combines in this diagram [what is] the inductances which are marked in Chaffee diagram No. 1, "L," and variable inductance. As long as the spark gap G is working this arrangement can be described as consisting of a primary circuit, containing the condenser C, the part PQ of the spiral S, the inductance L^1,
10680 and the spark gap, and of a secondary circuit, C^1, inductance L, spiral S, condenser C. But this view is entirely arbitrary, as may be shown by Figure Z-2 (reproduced opposite).

In this Figure Z-2 I marked correspondingly to Figure Z-1 the condenser at the left as C, the spark gap as G, the condenser at the right as C^1, the inductance at the right as L, the spiral as S, the left part of it as PQ, the right part of it as QR, and the small inductance as L^1. In Figure Z-2 the elements and their connection is exactly the same as in Z-1. In looking at this Figure Z-2

Z 4

July 25 16

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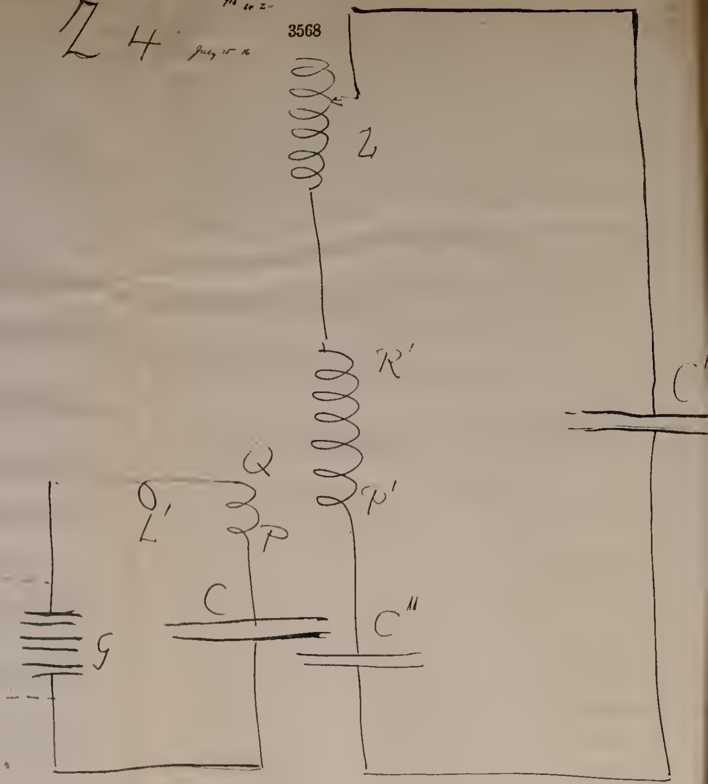

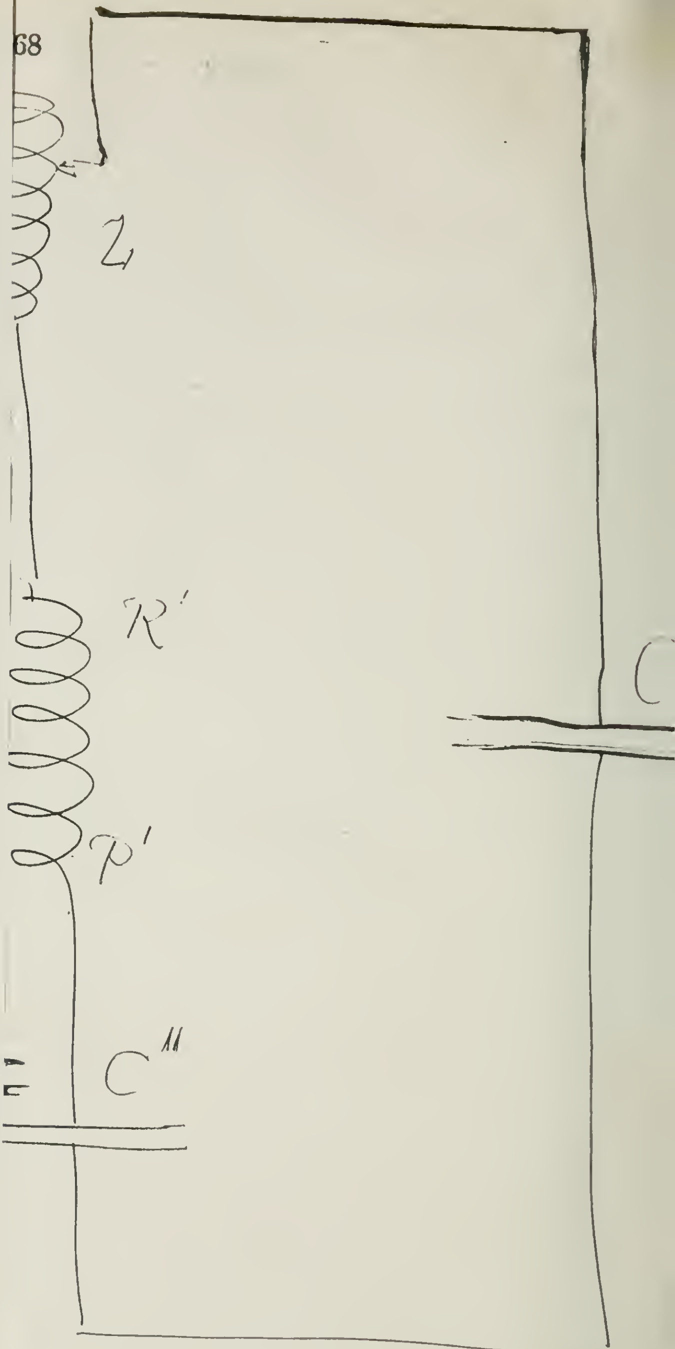
L
R'
p'
C''

 10705

it seems to be by far more natural that the two circuits to be considered are the circuit C, PQ, L^1, G, and the circuit C^1, L, RG, L^1, G.

The Figure Z-2 shows further that the question which of these two circuits is the primary and which is the secondary, is somewhat embarrassing. The figure itself does not give any indication as to that. Further, if we connect one of these two condensers, for instance C, with a feeding circuit, which I will mark by two dotted lines on Z-2, and charge it, both condensers are charged at the same time and to the same potential. Also, in this respect, there is, therefore, no difference between these 10706
two circuits. The only excuse for calling the circuit at the left the primary circuit would be that in the Massachusetts tests the capacity of the condenser C was materially greater than the capacity of the condenser C^1. It could therefore be said that we might call this the primary circuit, because initially the greatest part of the energy is contained in this circuit.

Q. The lefthand circuit? A. In the lefthand circuit. But this excuse does not exist if we consider the connection of the arrangement, so that we call primary circuit the circuit C, PQ, L^1, G.

Q. As now written on Z-3? A. As written on Z-3, 10707
(reproduced opposite) and the secondary circuit the circuit C^1, L, the spiral RP, or S, and condenser C. From this point of view the condenser C with the big capacity is common to both circuits. Its energy, therefore, belongs just as well to the so-called secondary as to the so-called primary circuit. There is not the least reason why the energy of this condenser should belong only to this circuit.

Q. Which circuit? A. To the circuit marked 1 on chart Z-3. The situation is different from that which I am going to represent on Z-4—when in the arrangement Z-4 (reproduced opposite) the condenser C is connected with a feeding circuit and thereby charged, the condensers C^1 C^{11} are not charged. There is no conductive connection between these two circuits. Here, theerfore, we are entitled to consider the circuit C, PQ, L^1, G as the primary circuit, as only this circuit initially contains energy.

(Whereupon the court takes an adjournment until July 25, 1916, at the hour of 9:30 o'clock a. m.)

10709

Tuesday, July 25, 1916, 9:30 A.M. Continuation of proceedings pursuant to adjournment, all parties present as at former hearing.

Jonathan Zenneck, resumes the stand for further direct examination.

The Court: Proceed.

The Witness: (Continuing his testimony) In systems like that used in the Massachusetts tests,
10710 these initial conditions have to be carefully taken into consideration. I called attention to that expressly on page 91 of my small book, where, in discussing the amplitude and phase of the oscillations of magnetically coupled circuits, I made a note, 91, referring to page 415, where I stated,

"What follows holds true only under the following initial conditions; when $t=O$ $V_1=V_{10}$, $I_1=O$; $1_2=O$."

Under other conditions in fact it may happen that only one oscillation occurs. The latter showing that there may be very important differences

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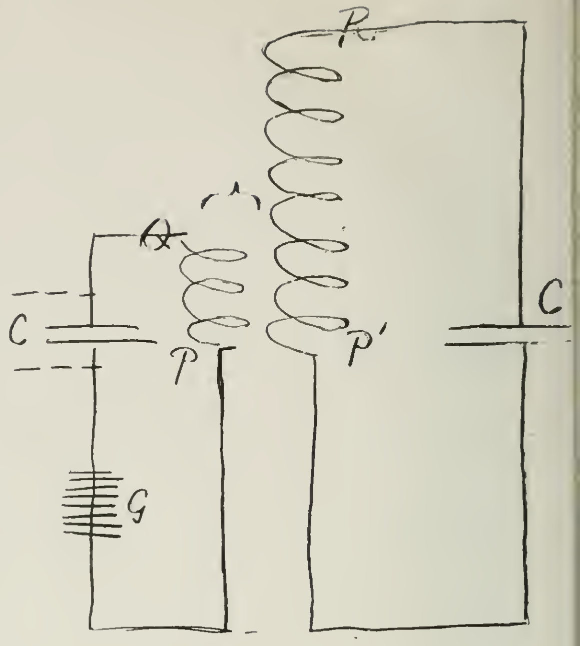
R
A
C
P
P'
C
G

 10717

according to the initial conditions. This difference of the initial conditions is to be considered also in the case which has been discussed here on one of the last days, regarding the Massachusetts tests; the question whether conductive coupling is identical with magnetic coupling. I have drawn in chart Z-5 (reproduced opposite) two circuits, or two systems; that represented in Fig. 1 conductively coupled; the one represented in Fig. 2 magnetically coupled. These two systems are practically identical, supposing, of course, the constants to be properly 10718
chosen; they are practically identical because the initial conditions are the same. When the condenser C is charged by connecting it with the feeding circuit, the condenser C^1 remains uncharged, and the same is the case herein Fig. 2. But as soon as we transfer the spark gap g from the point where it is in Fig. 1 to the point where it is in Fig. 3, this conductively coupled system, represented in Fig. 3, is no more identical with the magnetically coupled system of Fig. 2, because the initial conditions are different. When in this case the condenser C is charged by connecting it with the feeding circuit the condenser C^1 is 10719
charged at the same time and to the same potential.

The Witness: (Continuing) In such cases as the transmitter used in the Massachusetts tests, which may be looked upon from different view points, the best method to get a clear idea is always to go back to the mathematical theory, which is always the most exact expression of the facts. For this the situation is clear. In a transmitter like that used in these tests we have two phases. The first phase when the spark gap is conductive

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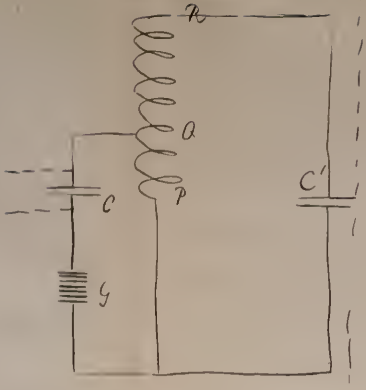

Fig (1)

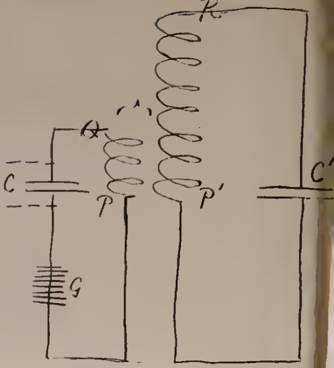

Fig 2.

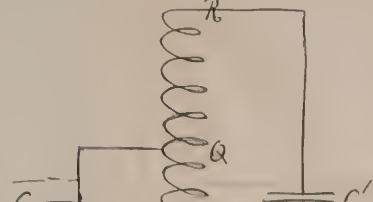

Fig.(3)

and in operation; the second when the spark gap is quenched. For the first phase, in addition to the initial conditions we get the equations containing the capacities of our condensers, the mutual and self-inductances of our coils, and the resistances of the leads and the spark gap. The result of these equations is exactly the same, no matter what we call primary or secondary circuit, and regardless of whether we rely upon the diagram represented in chart Z.1 or of that represented in chart Z.2. In the second phase, the lead
10721 containing the quenched gap is open. Everything happens as if these leads were disconnected. There is, therefore, only one oscillating circuit C^1, L, S, C, and we get an equation containing the constants of this circuit and, therefore, the free oscillations of this circuit.

In order to finish this question I may already state here that the Massachusetts tests have shown that in the transmitter tested there, the second phase is enormously longer than the first. It contained about 77 complete oscillations, the first only about two and one-half. This second phase is the characteristic feature of impulse transmitters like
10722 that tested in the Massachusetts tests. It is entirely or practically missing in the coupled circuit transmitter such as used during about the first ten years of wireless telegraphy; this transmitter does, therefore, practically not emit free oscillations of the antenna.

Section 4. The adjustment in the Massachusetts tests.

In order to adjust the set, Dr. Chaffee varied the variable inductance in the dummy antenna until the ammeter inserted into this dummy antenna showed a maximum indication. He did not make the adjustment by measuring accurately the frequency of the dummy antenna and the frequency of what he called the primary circuit, and making these frequencies exactly equal. Nor did he make the adjustment by measuring the capacity and inductance of the dummy antenna and of that circuit which he called the primary and making the product of those two quantities equal in both circuits. He just tried out when he got the maximum [effect—maximum] current in the dummy antenna.

Section C. The measuring arrangement. 10724

Besides the Braun tube with the auxiliary apparatus necessary for its operation, the measureing arrangement of Dr. Chaffee consisted in a device for the magnetic deflection of the cathode rays and in one for their electric deflection.

Section 1. The first arrangement for the magnetic deflection was formed by two leads which connected the coils marked e in Chaffee's diagram 1, (Page 2967) with two points S1 and S2 of the transmitter. This lead and the coils were, therefore, in shunt to the lead S1-S2, the current acting on the Braun tube was, therefore, the current in this shunt. This current was measured by 10725
the image on the screen of the Braun tube, not the main current in the lead S1-S2 itself.

The question, therefore, remains: "Which was the relation of this current measured by the image on the screen of the Braun tube and the main current flowing through the lead S1-S2?" It would be very hard to answer this question definitely, as not only the resistance of the shunt comes in, but also the inductance and the capacity, which in the Massachusetts tests was certainly not extremely small, as the leads connecting the coils

10726

e were about eight feet long, were very close together, in fact separated only by the insulating material of the wires, the dielectric constant of which was certainly considerably [higher and] greater than that of the air. The difficulties, as just mentioned, are the reason why such shunt connections, which are very generally used in direct current work, have a somewhat bad reputation [in radio telegraphy or, I will say] in radio work.

Furthermore, the coils e were so located that
10727 they were opposite to the plates inside the Braun tube and so that their plane was parallel to these metallic plates inside the tube. There must have been, therefore, a considerable production of eddy currents in the metallic plates, which also may have affected the magnetic field inside the Braun tube, and affected the cathode rays of this tube. It would have been possible to avoid this complication by putting the deflecting coils higher or lower than the deflecting plates.

Section 2. The object of the second part of the measuring arrangement was to actually produce curves of the current in the coils on the screen
10728 of the Braun tube. For this purpose Dr. Chaffee employed a method published in 1908 by L. Mandeistam, to which reference is made in my small book, on page 408, note 2 (showing book to court). This method makes use of an aperiodic discharge of a condenser circuit, namely, the condenser circuit marked c^{11} R2 in Chaffee's diagram. This aperiodic discharge of a condenser circuit means the following: When we increase the resistance of any condenser circuit, such as the condenser circuit just mentioned, all other constants and the form of the circuit remaining the same,

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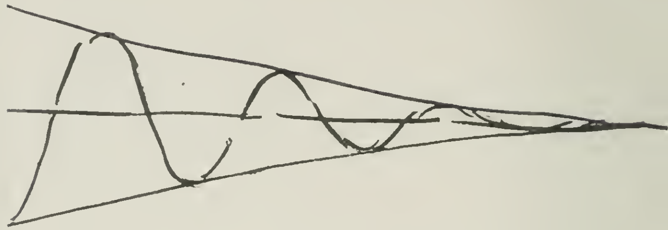


 10735

the number of oscillations is more and more reduced, and finally we get a single discharge in one direction. I have shown these successive stages, on chart which I will mark "Z-6" (reproduced opposite).

The Court: Isn't that "Zenneck Chart 2" now?

Mr. Farnsworth: This will be No. 6, your honor.

The Court: I thought that was the same one.

The Witness: (Continuing) These oscillations here in Fig. 1 may correspond to some value of
the resistance. If we increase the value of the 10736
resistance of the same condenser circuit, we get, for instance, a curve like this represented in Fig. 2, consisting only of say one and one-half complete oscillations, and if we increase the resistance still more we get a discharge like this here (showing) on Fig. 3, a discharge only in one direction. This is called the aperiodic discharge of such a condenser circuit. I have reproduced in my big book photographs made by means of the Braun tube, which exactly show what I have here represented on chart Z-6. The photographs I refer to are on page 360, Fig. 342; the next stage is on
page 385, represented on Fig. 362, the aperiodic 10737
stage on Fig. 363. The photographs of Dr. Chaffee do not show very clearly whether he has made use of a real aperiodic discharge or of a discharge such as represented on chart Z.6 Fig. 2—the second one. But it is immaterial for his tests, as the first part of both of those curves is not materially different.

To sum up, I may state the following; in discussing the Massachusetts tests I disapproved of several points in Dr. Chaffee's arrangement. I

Z 6 3578

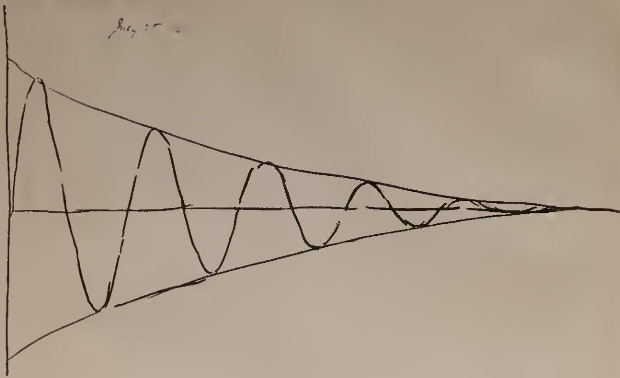


Fig. 1

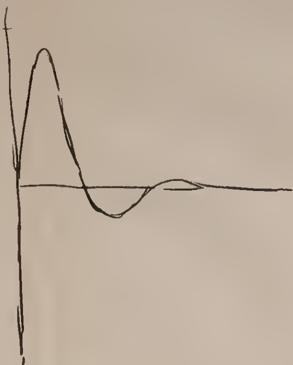


Fig 2

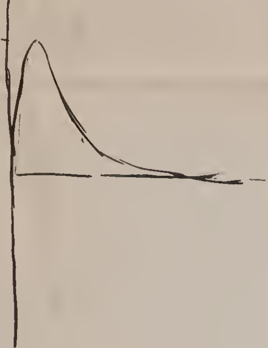


Fig (3)

10738 do not want to be understood as meaning that they have necessarily falsified the results of the Massachusetts test. I should, perhaps, not have disapproved them at all, if the object of those tests had been to state whether there was one-half an oscillation, or say, twenty oscillations. But the whole question concerned a relatively very slight difference; namely, whether there were one-half or two and one-half oscillations. Further, according to my experience, the quenching of a quenched spark gap is liable to be affected by relatively
10739 slight changes in the conditions. Dr. Chaffee seems to have had the same experience. He has testified, on page 44 of his testimony, that he sometimes got beats, that is to say, imperfect quenching effect, or no quenching effect at all, without any change in the adjustments of the set, only in consequence of the raise of temperature of the spark gaps.

The situation, therefore, seems to me the following: Dr. Chaffee should have avoided the points of which I disapproved, or should have shown us that they have not had any appreciable effect, in order to make his tests conclusive. That
10740 is all.

Q. (Mr. Farnsworth) What is, in fact, shown by the photographs which were taken at the plaintiff's Massachusetts tests on July 3rd and 4th of the Simpson mercury valve transmitter? A. Section A. I should first refer to the photograph marked BA and BC. These photographs show beyond any doubt, that the magnetic field inside the Braun tube was oscillatory and made about two and one-half complete oscillations. In view of the eddy currents in the plates inside the tube and of the deflecting coils e being shunted to a part of the spark circuit, I had expressed some doubts as to whether this proves that the current in the spark circuit has made two and a half oscillations. But I will assume it to be so, as I do not think it to be very material whether this number is exactly correct or not.

B. I further refer to the photographs BI and BM, which represent the current in the dummy antenna. These photographs seem to show that during about two and one-half oscillations the amplitude in the dummy antenna increases, and then gradually decreases.

Comparing this with the results obtained for the spark circuit and showing that there were about two and one- 10742
half oscillations, I conclude that as long as there was a current in the spark circuit there was an increase in the amplitude of the current in the dummy antenna. After about two and one-half oscillations, according to the photographs BA and BC just mentioned, the spark was quenched. The lead, marked in Chaffee's diagram 1, "L[1], G", was electrically disconnected, as no current was flowing through it. There was, therefore, only one system, the dummy antenna, and only one kind of oscillations, the free oscillations of this antenna.

Owing to the fact that the velocity imparted to the cathode rays inside the Braun tube, in consequence of their electrical deflection, was not constant, a fact which 10743
has been fully explained by Dr. Chaffee—these photographs BI and BM, may easily give a somewhat incorrect idea. It seems that Dr. Chaffee himself had been deceived, as he represented the result in the form of Chaffee diagram 9, which tends to show that the time during which the amplitude was increasing was a considerable percentage of the time during which oscillations of appreciable amplitude existed. I have actually calculated the curves, according to the photographs BI and BM, assuming the figures given by Dr. Chaffee for his dummy antenna, namely, a wave length of about six

10744

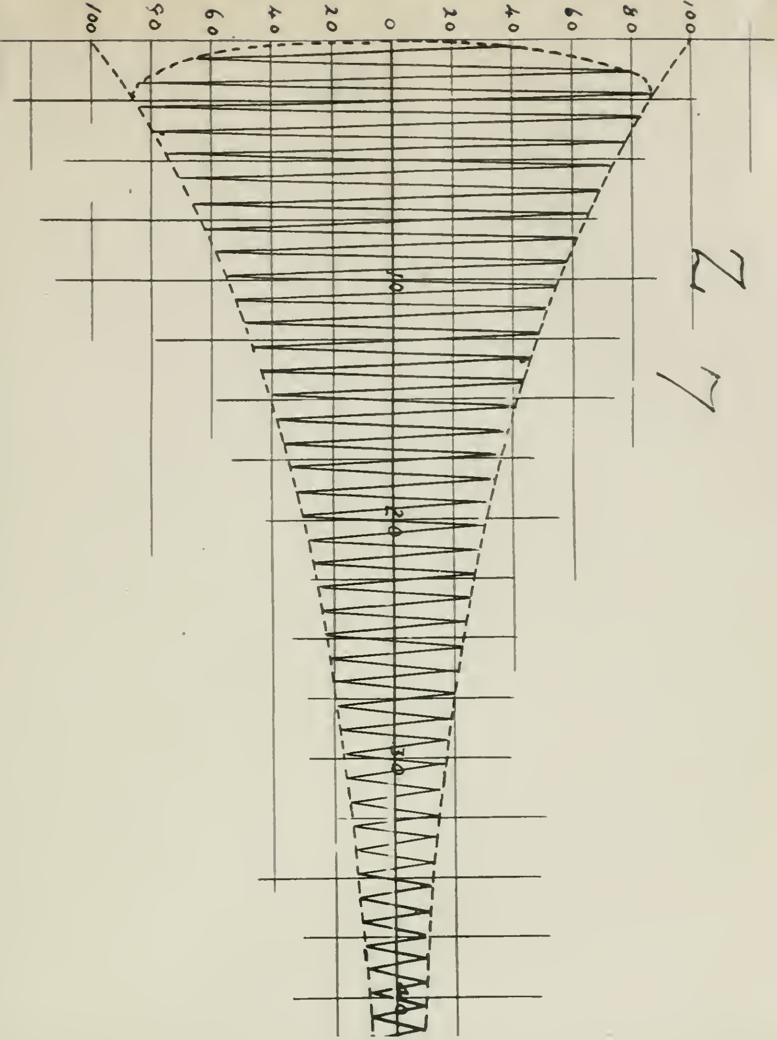
hundred meters, capacity C^1 equal to .001 microfarads, and the resistance of about six ohms, meaning a decrement of about .06, the result is represented on the chart which I mark "Z.7" (reproduced opposite). Assuming as a decrement the value .04 which had been measured in the tests made at the Bureau of Standards, and which corresponds to the figures given in the report of the Committee on Standardization of the Institute of Radio Engineers, pages 22 and 23, No. 1011, for a standard antenna of 600 meters wave length.

10745 (A report of Committee on Standardization, Institute of Radio Engineers for 1915, introduced in evidence as "Defendant's Exhibit No. 61.")

A. (Continuing) Assuming these figures, the corresponding curve is represented on chart Z.8 (reproduced opposite). These curves show very clearly that after an extremely small percentage of the entire oscillation time, we get the free oscillations of the antenna.

If we compare these oscillations represented on charts Z.7 and Z.8 with the oscillations of a system oscillating from the beginning with its free oscillations, not only after two and one-half oscillations—I have represented
10746 these oscillations in figures which I mark "Z.9" (reproduced opposite) corresponding to a decrement of .06, and "Z.10" (reproduced opposite) corresponding to a decrement of .04—it may be that we would hardly become aware of the difference between these figures if our attention had not been called to it.

I may add the following: The Committee on Standardization of the Institute of Radio Engineers, in its report for the year 1915, defendant's Exhibit 61, page 14 of No. 61, has defined "impulse excitation" as follows: "A method of producing free alternating currents in an excited circuit in which the duration of the excit-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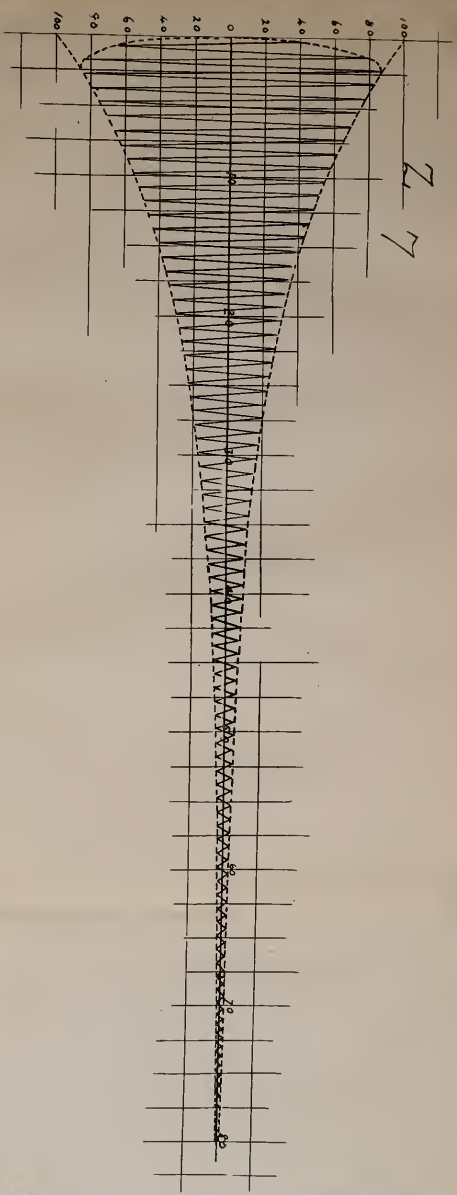

hundred meters, capacity C^1 equal to .001 microfarads, and the resistance of about six ohms, meaning a decrement of about .06, the result is represented on the chart which I mark "Z.7" (reproduced opposite). Assuming as a decrement the value .04 which had been measured in the tests made at the Bureau of Standards, and which corresponds to the figures given in the report of the Committee on Standardization of the Institute of Radio Engineers, pages 22 and 23, No. 1011, for a standard antenna of 600 meters wave length.

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A. (Continuing) Assuming these figures, the corresponding curve is represented on chart Z.8 (reproduced opposite). These curves show very clearly that after an extremely small percentage of the entire oscillation time, we get the free oscillations of the antenna.

If we compare these oscillations represented on charts Z.7 and Z.8 with the oscillations of a system oscillating from the beginning with its free oscillations, not only after two and one-half oscillations—I have represented
10746 these oscillations in figures which I mark "Z.9" (reproduced opposite) corresponding to a decrement of .06, and "Z.10" (reproduced opposite) corresponding to a decrement of .04—it may be that we would hardly become aware of the difference between these figures if our attention had not been called to it.

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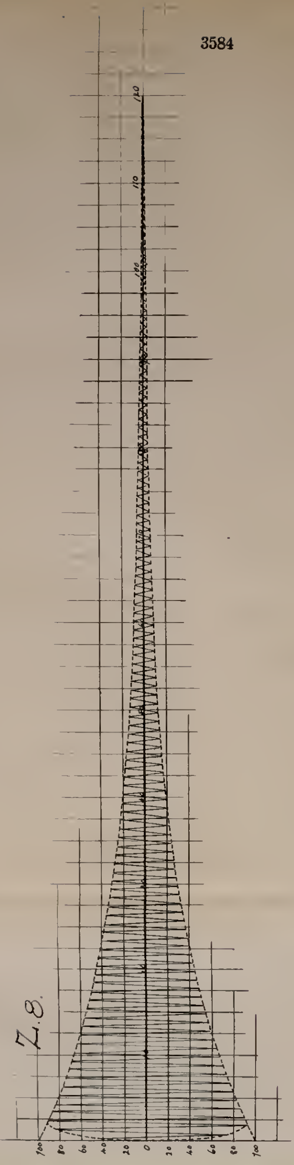
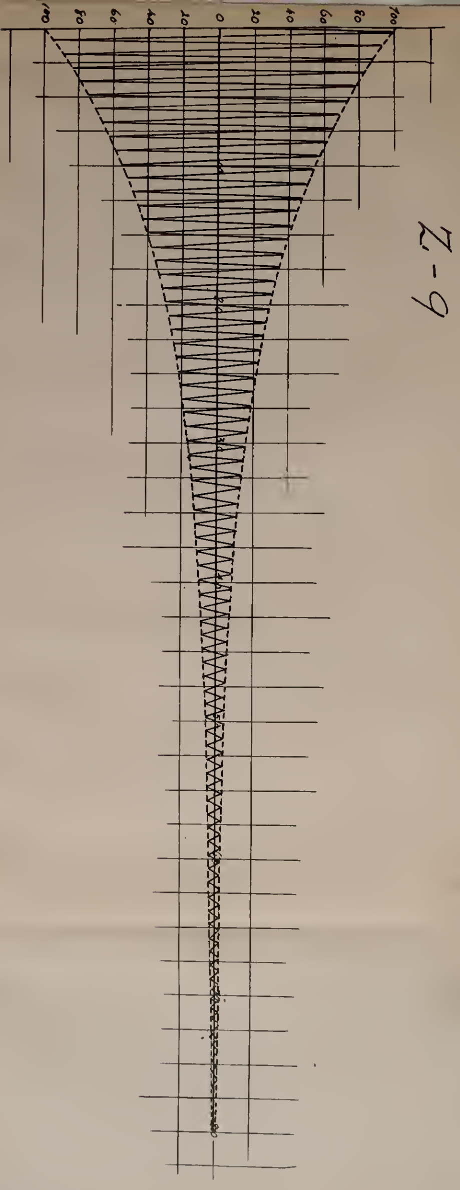


Z. 8.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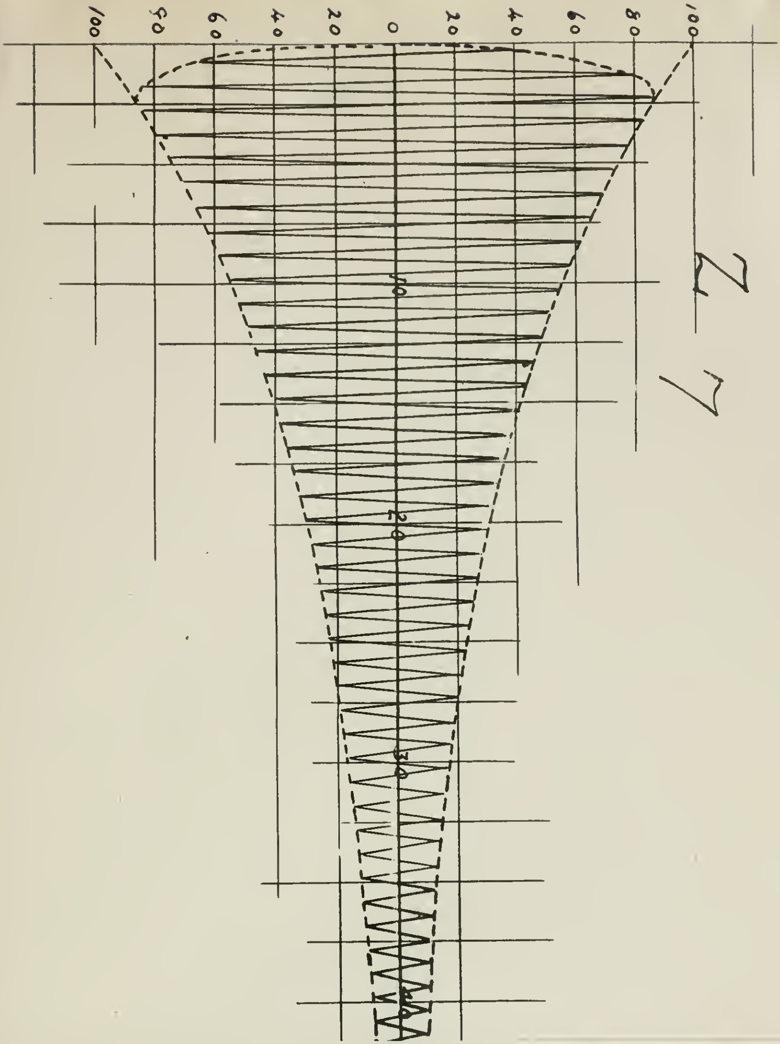
Z-9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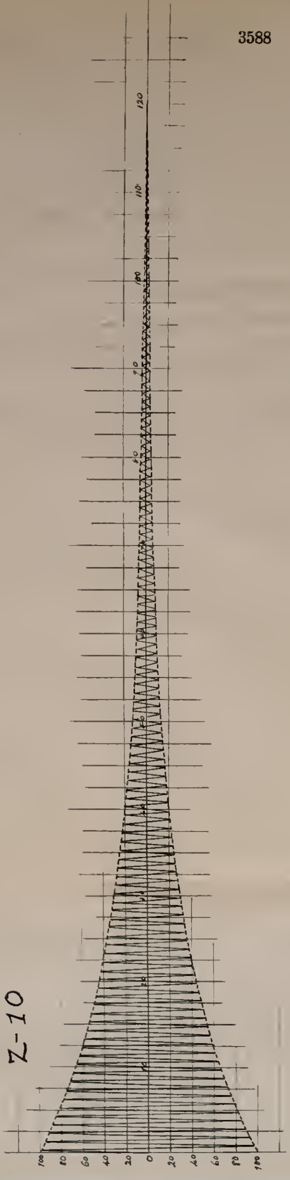
Z-10
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110
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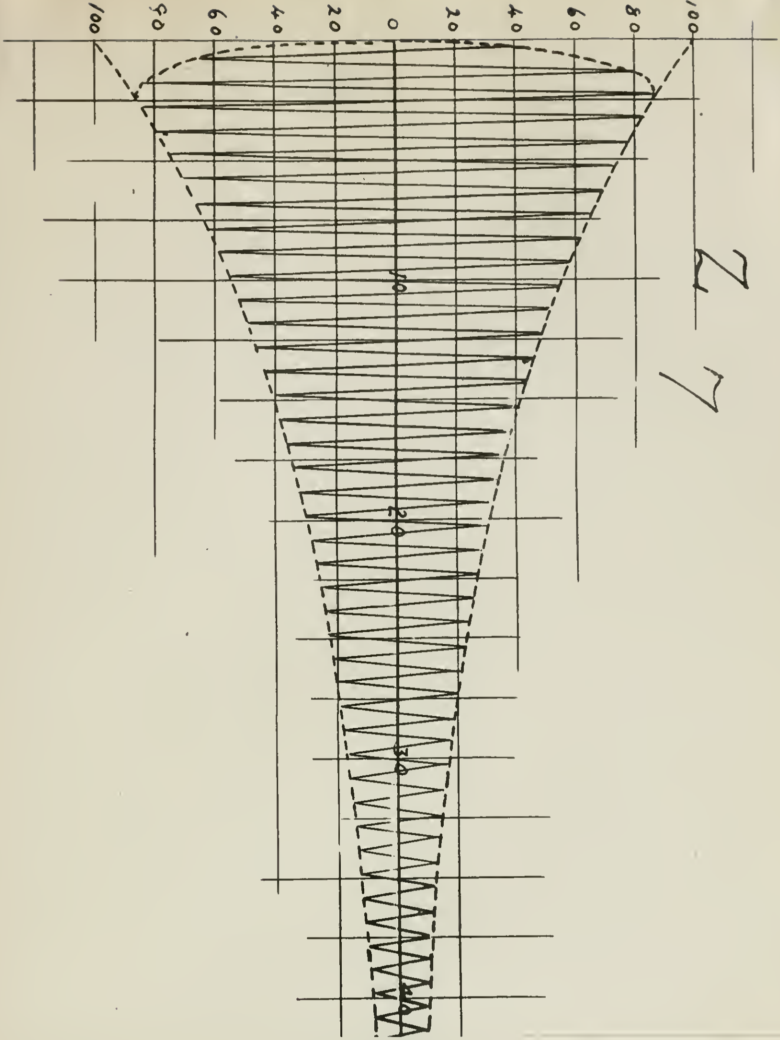

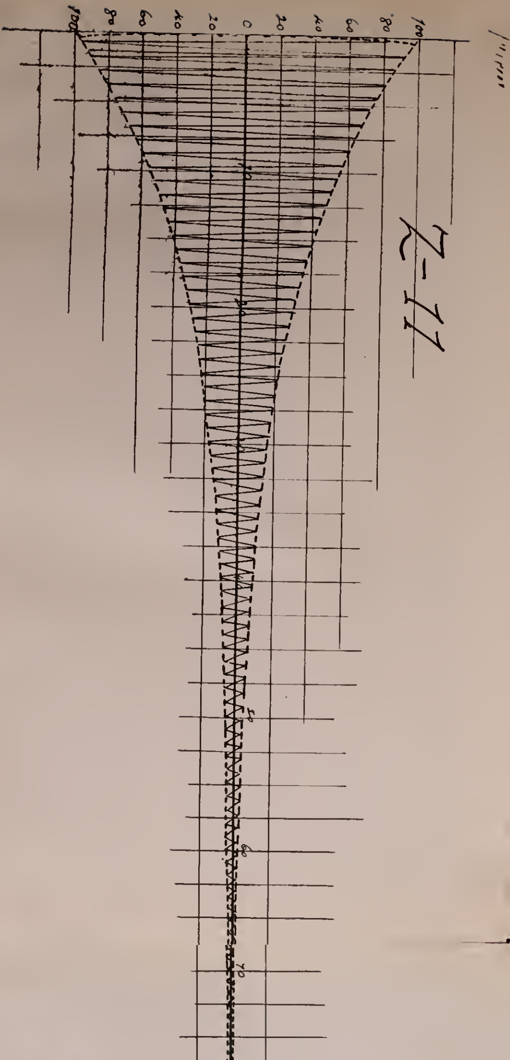

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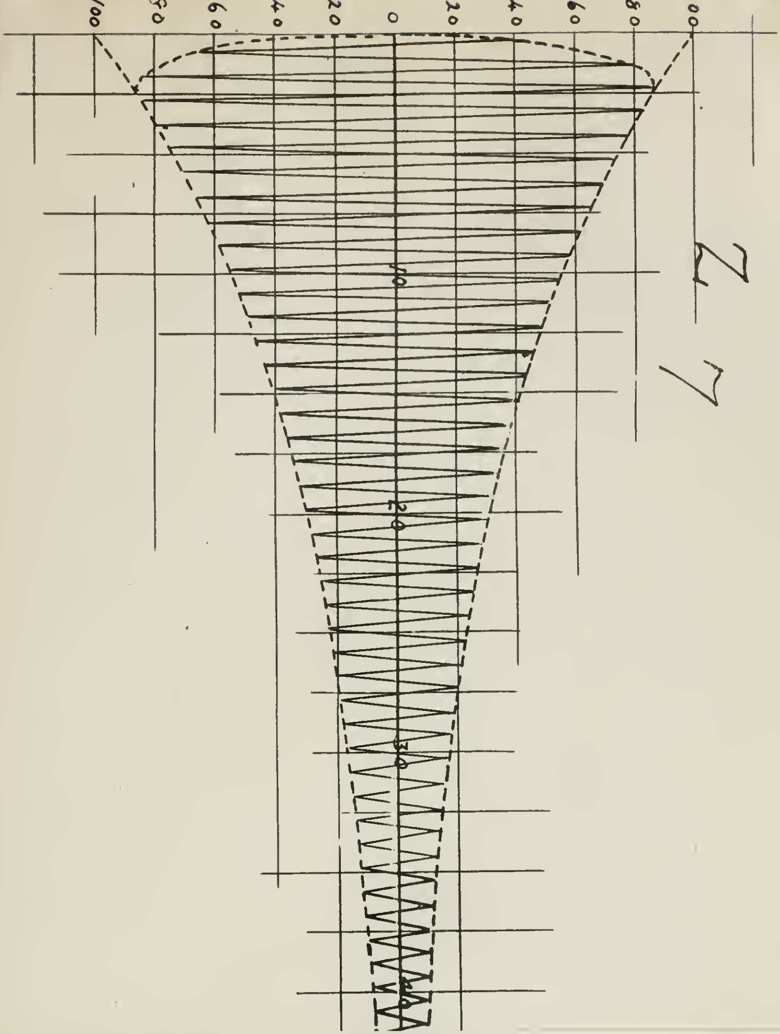

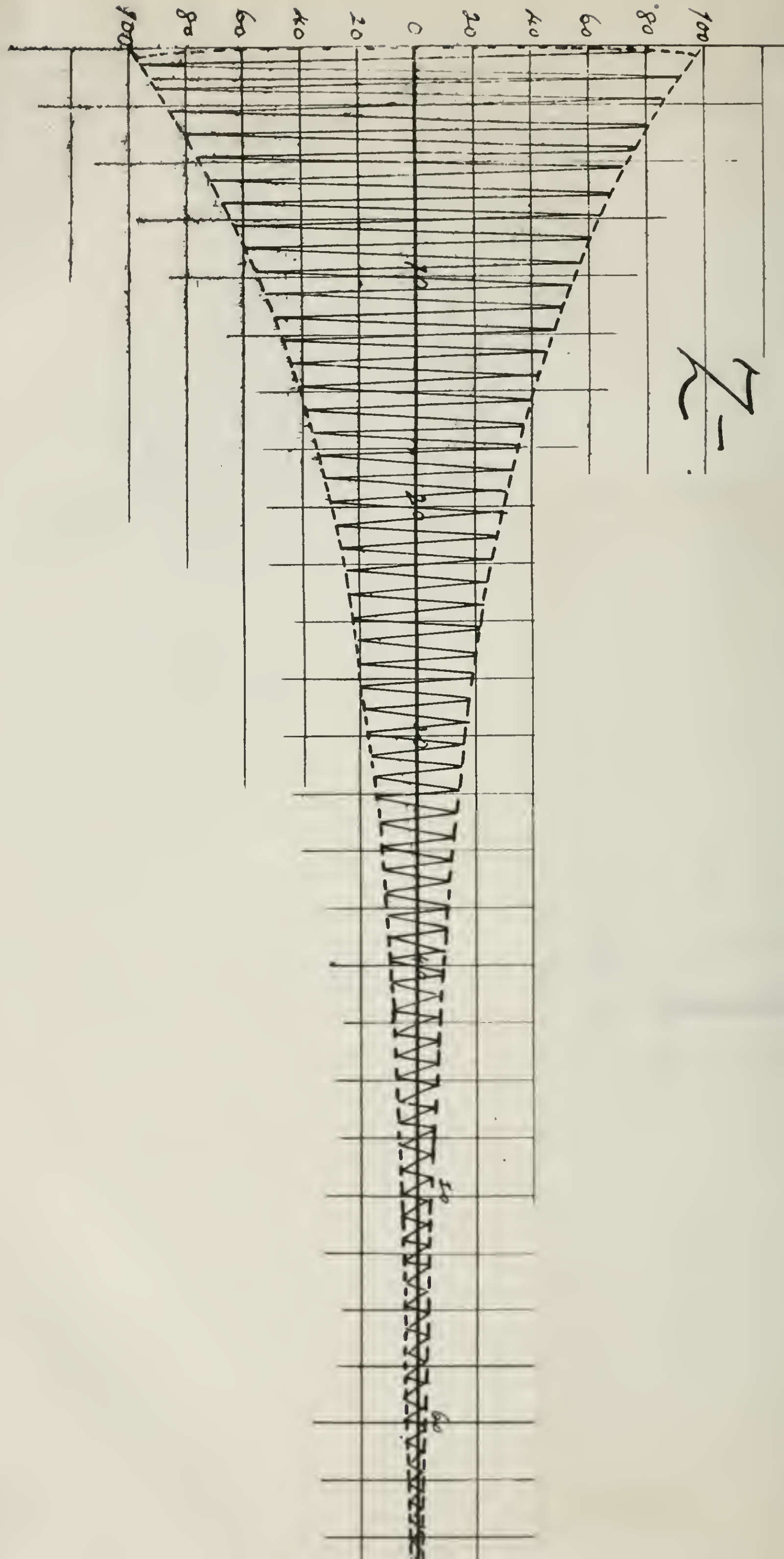
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 10771

ing current is short compared with the duration of the excited current." This definition has again been agreed upon in one of the last meetings. I fully agree with this definition, and think, therefore, that the oscillations represented in Figs. Z.7 and Z.8 represent a very good example of impulse excitation. There is no question that the impulse excitation would be still purer if the free oscillations were already present after one-half oscillation. I have shown this case in Fig. Z.11 (reproduced opposite) for a decrement of .06, but comparing this figure with the corresponding figure Z.7 for the same
decrement, it seems to me that the difference between 10772
these two figures is extremely small. From the standpoint of a physicist, therefore, and considering the result, it seems to be very immaterial whether the free oscillations start after one-half an oscillation or after two and one-half.

C. I finally refer to photograph 60, which represents the free oscillation of the circuit which I have marked on chart Z.1 as C, P Q, L¹, G, all other parts represented in this Fig. Z.1 having been disconnected.

This shows about six complete oscillations. This is, therefore, the greatest possible number of oscillations which can be obtained in this circuit which does
not radiate any appreciable amount of its energy. 10773

Mr. Betts: Are you referring to the spark gap circuit or the antenna circuit in your answer; you said, "this circuit," do you mean the spark gap circuit?

A. I mean the circuit which I have represented by C, P Q, L¹, G.

Mr. Betts: On the chart Z.1, the spark gap circuit?

A. You may call it so, yes, the spark gap circuit.

They are obtained when the circuit does not transfer any energy to any other circuit, when, therefore, it is allowed to consume all its energy in itself.

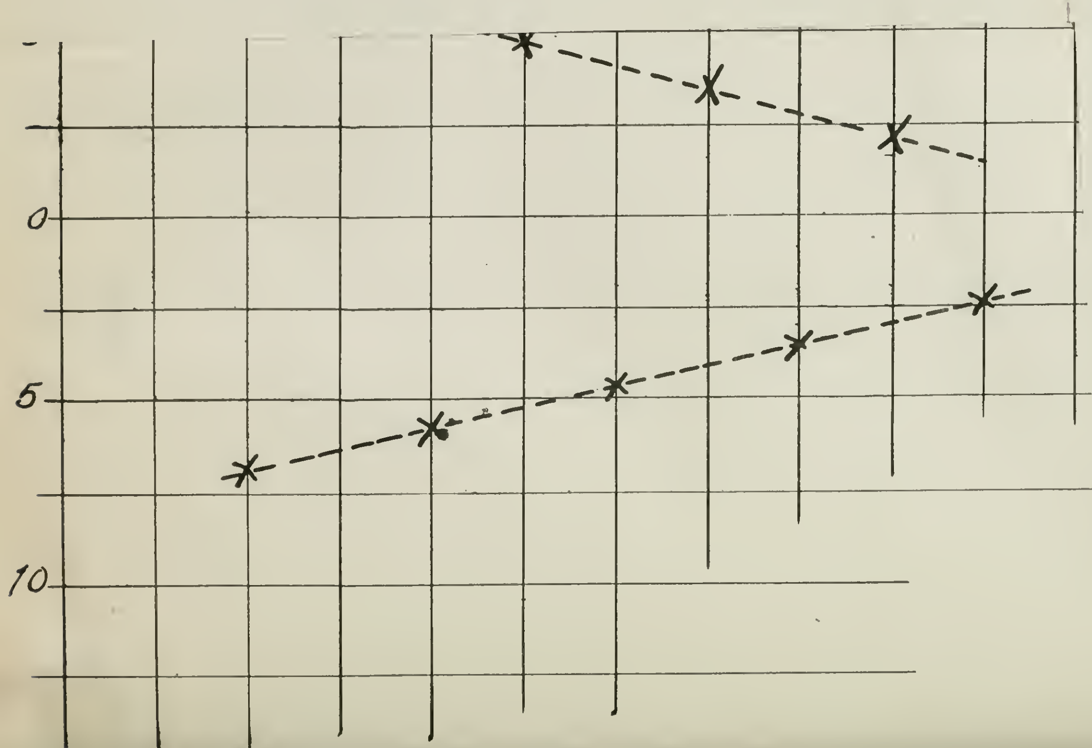
At which part of the circuit the energy is consumed can also be shown by this photograph 60. I have measured the amplitudes in this photograph as well as it can be done from such a photograph, and have drawn the amplitude curve, which I mark now "Z.12" (reproduced opposite).

This curve shows a practical linear decrease of the
10775 amplitude. According to what I have stated in Section 9, page 13 and following, of my small book, this means that by far the greatest part of the energy, if not practically all of it, is consumed in the spark gap. This consumption of energy in the spark gap, therefore, is the reason for the very small number of oscillations which can be obtained in this circuit.

This small number of oscillations obtainable in this circuit has nothing whatsoever to do with the ratio of the capacity of the circuit to the inductance of it. That is to say, with the ratio, as it has been called, C over L.

The formula, according to which the decrement is
10776 equal to $\pi R \sqrt{\frac{C}{L}}$ which has been cited from page 13 of my small book, is contained in Section 8, which is headed "Condenser circuit without spark gap." Furthermore, the audio frequency oscillograms in Fleming's book, on page 110, which have been also cited in this connection, also refer to condenser circuits without spark gap, as is clearly shown in the description, pages 28-29 of this book.

How the conditions are in condenser circuits containing a spark gap such as that to which the photograph 60 refers, is stated on page 17 of my small book: "It follows that within the limits for which the relation (1)



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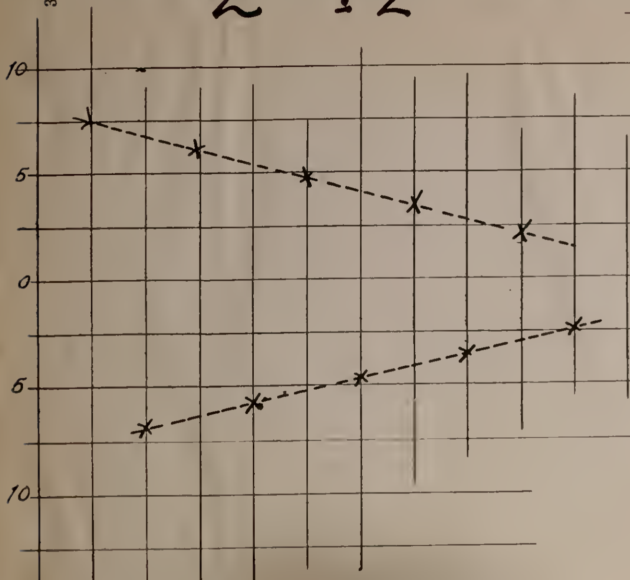
Z-12



Chaffee Phot. 60

Upper part	Lower part
7.5	7,0
6.1	5,7
4,6	4,6
3,4	3,4
2,1	2,3

A. You may call it so, yes, the spark gap circuit.

They are obtained when the circuit does not transfer any energy to any other circuit, when, therefore, it is allowed to consume all its energy in itself.

At which part of the circuit the energy is consumed can also be shown by this photograph 60. I have measured the amplitudes in this photograph as well as it can be done from such a photograph, and have drawn the amplitude curve, which I mark now "Z.12" (reproduced opposite).

This curve shows a practical linear decrease of the
10775 amplitude. According to what I have stated in Section 9, page 13 and following, of my small book, this means that by far the greatest part of the energy, if not practically all of it, is consumed in the spark gap. This consumption of energy in the spark gap, therefore, is the reason for the very small number of oscillations which can be obtained in this circuit.

This small number of oscillations obtainable in this circuit has nothing whatsoever to do with the ratio of the capacity of the circuit to the inductance of it. That is to say, with the ratio, as it has been called, C over L.

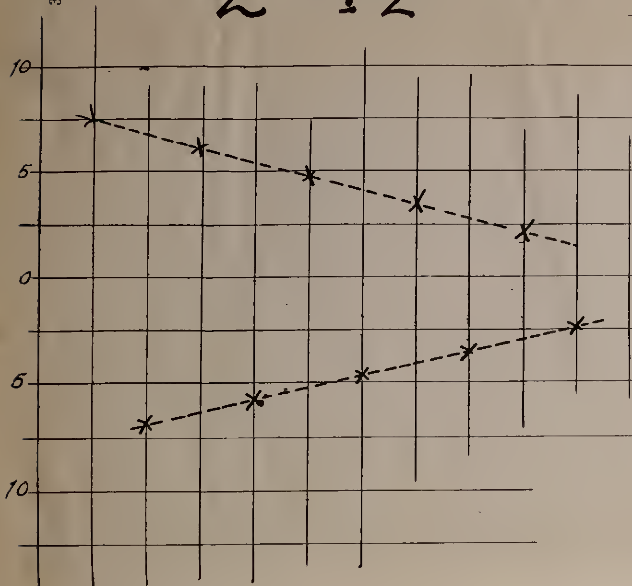
The formula, according to which the decrement is
10776 equal to $\pi R \sqrt{\frac{C}{L}}$ which has been cited from page 13 of my small book, is contained in Section 8, which is headed "Condenser circuit without spark gap." Furthermore, the audio frequency oscillograms in Fleming's book, on page 110, which have been also cited in this connection, also refer to condenser circuits without spark gap, as is clearly shown in the description, pages 28-29 of this book.

How the conditions are in condenser circuits containing a spark gap such as that to which the photograph 60 refers, is stated on page 17 of my small book: "It follows that within the limits for which the relation (1)

3593

Chaffee Phot. 60

Z-12



Upper part	Lower part
7.5	7,0
6.1	5,7
4,6	4,6
3,4	3,4
2,1	2,3

10774 Dr. J. Zenneck—Recalled—Direct.

A. You may call it so, yes, the spark gap circuit.

They are obtained when the circuit does not transfer any energy to any other circuit, when, therefore, it is allowed to consume all its energy in itself.

At which part of the circuit the energy is consumed can also be shown by this photograph 60. I have measured the amplitudes in this photograph as well as it can be done from such a photograph, and have drawn the amplitude curve, which I mark now "Z.12" (reproduced opposite).

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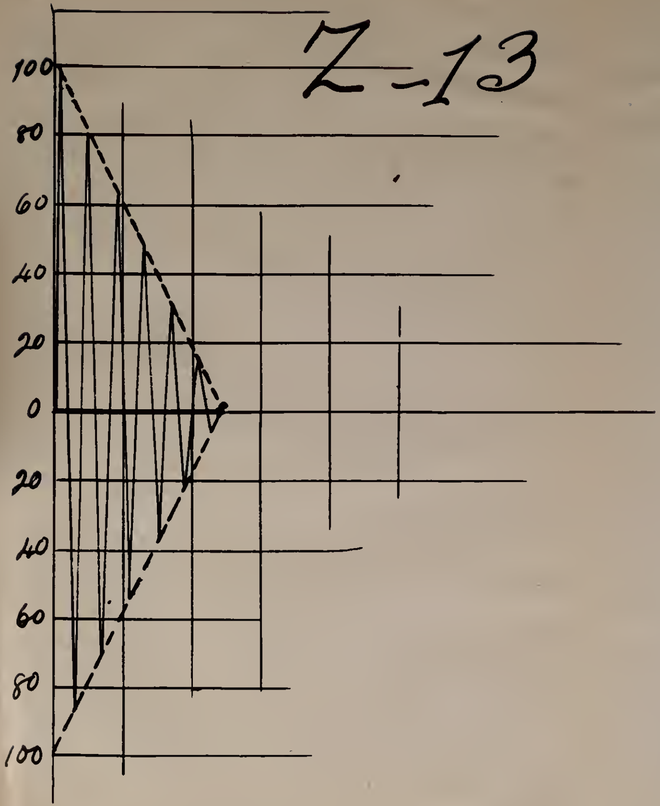
Z-13
100
80
60
40
20
0
20
40
60
80
100

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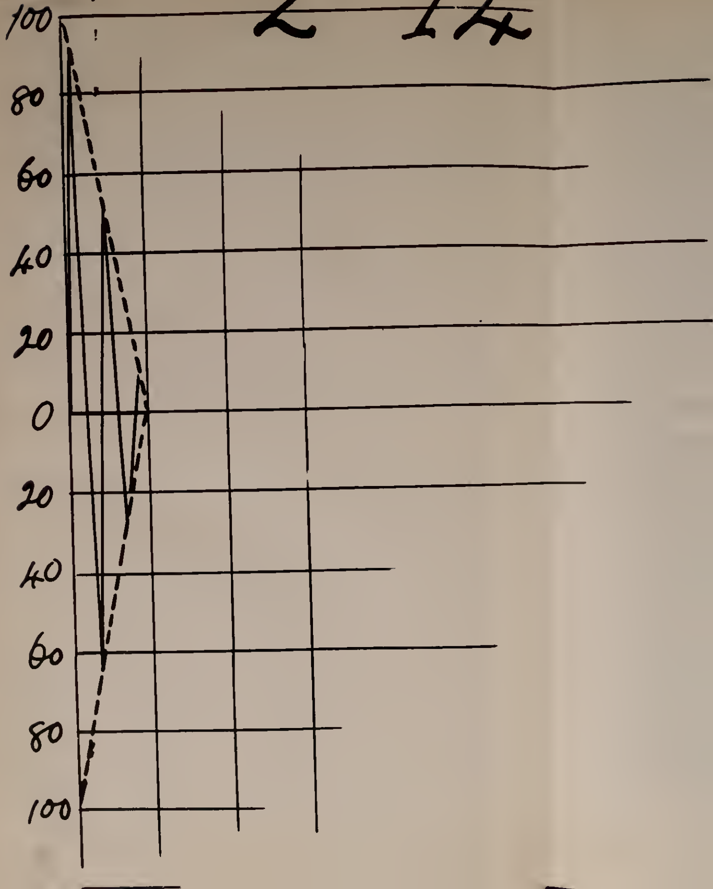
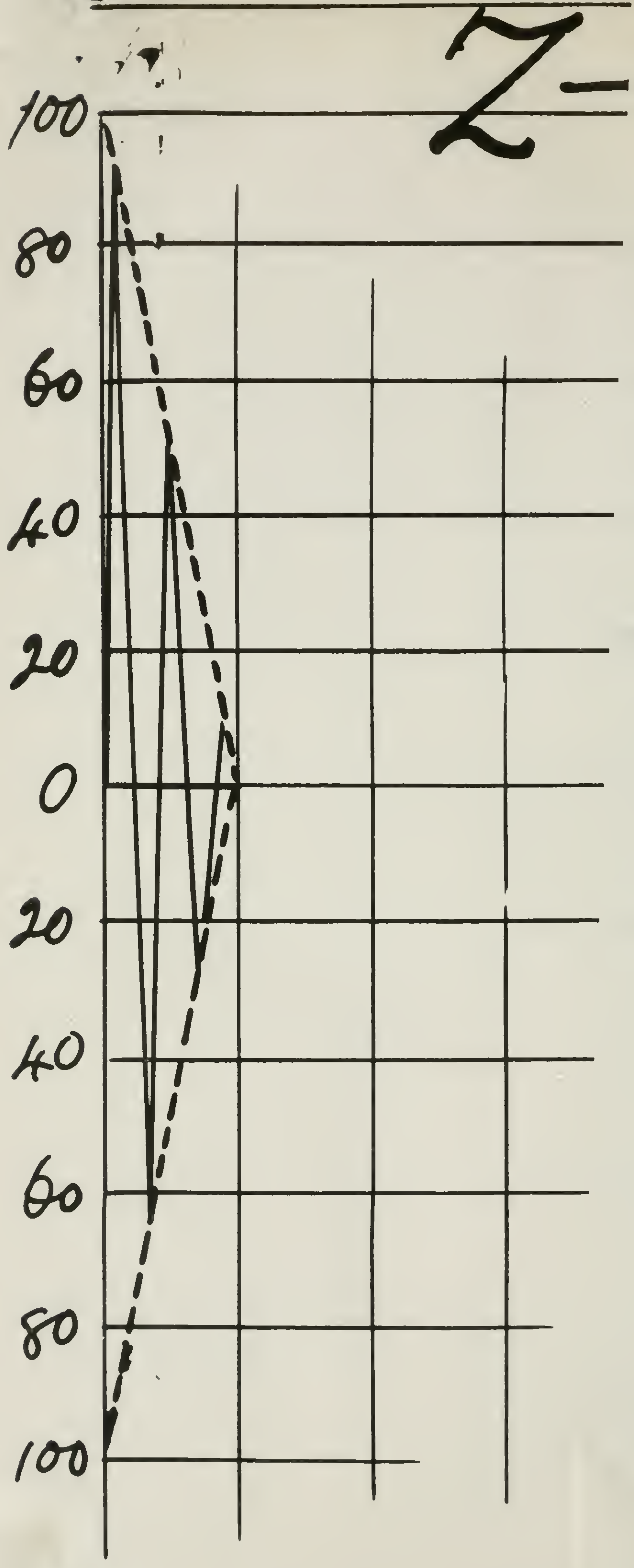
Z-14 3600




Z-
100
80
60
40
20
0
20
40
60
80
100

compare with them the oscillations of the coupled spark circuit, which is represented on chart Z-14 (reproduced opposite), and shows two and one-half oscillations. Then the difference gets still stronger, and I think still more that nobody would be willing to call the oscillator represented on Z-14 a persistent oscillator.

Q. Referring further to the condenser circuit of the Simpson transmitter used in plaintiff's Massachusetts tests, state whether or not, in your opinion, the question of the persistency of oscillation of such a circuit is determined by the form of the circuit, that is, by the circuit being in form either open or closed. A. I have de- 10802
fined persistent oscillator as an oscillator the oscillations of which persist for a relatively long time, and which has, therefore, a relatively large number of oscillations. I, therefore think that the question of the persistent oscillator is a question of the number of oscillations, and not of the form of the circuit. The Massachusetts tests in this respect have shown the following; namely, that the condenser circuit, the oscillations of which are shown in the photograph No. 60, although being of the closed type, could make at most six oscillations. I have further explained here, referring to figures in my big book, and referring to a periodic condenser of Dr. Chaffee, that in ony condenser circuit by increasing 10803
the resistance of it without channging in any way its form, the number of oscillations can be reduced to any number.

On the other side, in the report of the Committee on Standardization, which I have referred to already, for a standard antenna for a wave-length of 600 meters there is given a resistance of 4 ohms, corresponding to a decrement of about .04, which means that the free oscillations of such an antenna belonging to the open radiating type is of the form represented on chart Z-10, meaning that only after about 115 oscillations the amplitude of the oscillations gets down to one per cent of the initial amplitude. This figure shows, therefore, very clearly that an oscillator, although being of the open type, may be a very persistent oscillator. We would get to the same result if instead of assuming a decrement of .04 we would assume the decrement which corresponds to the figures given by Chaffee. The oscillations are then represented on chart Z-9, and still mean a persistent oscillator. In this case the amplitude would be one per cent of the initial amplitude after about 77 oscillations.

10805 It is therefore a fact that the number of oscillations in a condenser circuit being of the closed type can be made very small and, therefore, the condenser circuit made a non-persistent oscillator. On the other hand, the number of oscillations in an antenna, although being of the open type, can be very large, the oscillator, therefore, very persistent. I draw from this the conclusion that the number of oscillations is not determined by the form of the circuit.

The opinion that the question of the persistent oscillator has nothing to do with the number of its oscillations, but is solely determined by its form, seems to me to come down to the following statement: the question of the per-
10806 sistent oscillator is independent of whether its oscillations persist or not.

Q. Recently, on July 22 and 24, the defendant made some receiver tests which I understand as follows: The receiver consisted of two circuits, as follows, first, an antenna, the period of which was variable and was varied to the different received wave-lengths; second, a detector circuit containing no variable inductance coils and no variable condensers, those two circuits being coupled together by an inductive coupling. In the tests waves were

 10807

efficiently received having wave-lengths from 300 meters to 3600 meters, the coupling between the circuits being varied for the reception of the different wave-lengths. Assuming the correctness of that statement, and without going into details, what can you say of that result which was obtained without any variation of inductance coils or condensers in the detector circuit? A. I understood it. I can only say that this result is not unexpected to me. I have described in my small book on page 313 such a receiver, as follows:

> "If the decrement of the antenna is not much different from that of a well designed condenser circuit without spark gap, then the use of a condenser circuit as secondary no longer offers the same advantages as with a strongly damped antenna. (Art. 180d). 10808
>
> Hence, in this case, which applies to all quenched spark operation, the antenna is coupled to a *closed* detector circuit containing the detector as shown in Fig. 375."

There is also a note:

> "C^1 is simply a block condenser of great capacity."

10809

This note, together with the fact that I distinguished a closed detector circuit from a secondary condenser circuit, shows that by closed detector circuit I meant a periodic detector circuit. I may state that I based the statement on two papers of F. Kiebitz and K. Bangert, which had been published in 1909 and 1912, and refer to receivers of this kind where the secondary circuit does not consist of an oscillating condenser circuit.

Mr. Farnsworth: If the court please, I will offer the exhibits as defendant's exhibits, the

10810

things referred to by the witness in his answers, including the charts and diagrams Z-1 to Z-14; also we would ask that the two books produced by the witness as written by himself be marked for identification as "Zenneck small book" and "Zenneck large book"; also the pages of said Zenneck books referred to by the witness be respectively offered in evidence as follows and properly numbered: The pages in the small book being pages 21, 138, 91, 408, 13, 18, and 313; the pages of the large book being 360 and 385. The report of the Standardi-
10811 zation Committee for 1913, referred to by the witness, has already been offered in evidence and marked as defendant's exhibit No. 61.

The Court: These charts are marked Z-1 to Z-14?

Mr. Farnsworth: Yes, Z-1 to Z-14, inclusive. You may cross examine.

Mr. Betts: I think both books referred to by the witness should be offered in evidence and not merely marked for identification.

The Court: I understand they were offered in evidence.

10812 Mr. Skeel: The books?

The Court: Yes.

Mr. Skeel: Yes, we would like to have the books in evidence, if Mr. Betts will agree. They should be marked as defendant's exhibits No. 62 and 63, and the marking of the pages will not be made necessary, since the witness has referred to them.

(Small Zenneck Book received in evidence and marked "Defendant's Exhibit No. 62".)

(Large Zenneck Book received in evidence and marked "Defendant's Exhibit No. 63".)

 10813

(Charts and blue-prints marked Z-1 to Z-14, inclusive, received in evidence and marked "Defendant's Exhibit No. 64".)

Cross Examination.

Q. (Mr. Betts) What is the well known Bjerknes method for determining the decrement of a circuit? A. The method consists in coupling extremely loosely with the circuit in question a so-called measuring circuit formed by a condenser circuit, the frequency of which
can be varied, and which contains an instrument which 10814
in some way allows the measuring of the effective current in the measuring circuit. The method then consists in plotting a curve, the abscissae of which are proportional to the frequencies of the measuring circuit, the ordinates proportional to the effective current in the measuring circuit. From this curve the sum of the decrements of the oscillating circuit and of the measuring circuit can be calculated.

Q. And if you were given the decrement of an oscillation wireless circuit could you by using the Bjerknes method calculate the number of oscillations in that circuit? A. You can do that only in one case, namely, if 10815
the decrease of the amplitude in the oscillating circuit, the decrement of which you want to measure, follows the logarithmic law.

Q. Could you, if you were given the decrement of a spark gap circuit in a wireless telegraph transmitter, calculate the number of oscillations in that circuit by using Bjerknes method? A. If you ask me that I would first ask you how did you measure the decrement of this circuit containing a spark gap, and what do you mean by saying the decrement is such and such?

Q. Is that all? A. That is all I can say now. May I add, that, you can calculate the number of oscillations by making some assumptions about the decrease of the amplitude in this circuit containing a spark gap. But these assumptions are always more or less arbitrary as long as the real decrease of the amplitude has not been measured, for instance, by using the Braun tube.

Q. In addition to writing the book in German which has been offered in evidence as defendant's exhibit No. 63——

Mr. Farnsworth: The large Zenneck book.

10817 Q. (Continuing)—have you written any other books in German on the subject of wireless telegraphy? A. Yes. I have mentioned in the direct examination that I have written a second book. The English translation of the second edition of this book has been marked defendant's exhibit No. 62.

Q. Dr. Zenneck, you remember testifying for the defendant Atlantic Communication Company in the suit brought by the Marconi Company upon the patent here involved, before Judge Veeder? A. Yes, I remember.

Q. And in the course of your cross examination there you remember translating for me certain passages from
10818 your book? A. I remember, yes.

Q. And you testified as follows, did you not:

> "Referring to your book, defendant's exhibit D-42, page 114, Section 64, I will ask whether the following is a correct translation of that section.
>
> The Court: May Dr. Zenneck not translate it?
>
> xQ. 168c. Yes, will you please translate Section 64 of your book? A. Yes.
>
> a. Considering that the primary system absorbs the less energy the quicker the oscillations in it are quenched, that is to say, considering the efficiency, it is favorable to use the tightest possible coupling. The tighter it is, the shorter is

the duration of one beat, and therefore the time during which the primary system remains in action. On the other hand, the time during which the amplitude in the primary system is very small, and therefore also the time which the spark-gap has for its de-ionization, becomes shorter, the tighter the coupling is. If it becomes too small, then no pure quenching effect takes place, that is to say, the oscillations of the primary system are not quenched already after half a beat. One gets either coupling-oscillations or a rather unstable intermediate case between the case of coupling-oscillations and that of pure quenching effect. The oscillations in the primary circuit cease only after 10820
one beat and a half, or two beats and a half. For a high degree of efficiency, a coupling as tight as possible is favorable; for realizing a pure quenching effect, loose coupling is favorable. Consequently for each given spark-gap, a 'critical coupling degree' must exist for which a pure quenching effect just takes place. This coupling degree will be always employed to obtain the highest possible degree of efficiency. The tighter it is the better is the quenching effect of the particular spark-gap.

b. Purity of beats. It is favorable for the quenching effect that after half a beat the amplitude of the resulting oscillation in the primary system really becomes zero, and therefore the beats 10821
are pure. The condition for that is that the two oscillations after half a beat have the same amplitude but opposite phase.

Whether this occurs depends, first, on the accuracy of tuning between primary and secondary system. The more exact it is, the purer is the beat, under otherwise identical conditions. And then even with exact tuning, as may be easily seen, the purity of the beats depends on the initial amplitudes of the two oscillations, and on their decrements. In this respect also the decrements of the primary and secondary system are of importance. As the decrements of the coupling-oscilla-

10822 tions are further dependent on the coupling degree, the coupling degree also in this respect has an influence on the quality of the quenching effect.

It is likely that this influence of the coupling degree is of importance for the following experiments.—(H. Riegger.)

XQ. 168d. I am not interested in that passage which is referring to another book, but merely what you said in your book. A. Yes. (Continuing translation): If one increases the coupling degree continuously, one gets a first critical coupling degree, which being exceeded a pure quenching effect is no more obtained. If the coupling is made still
10823 tighter, there occurs for another definite coupling degree another pure quenching effect. (Second critical coupling degree.) Under certain conditions, this effect can take place again for a third coupling degree. The critical coupling degree of a quenched gap is therefore by no means always fully determined.

Also, for the fact that under certain conditions, by a small de-tuning of the two systems again pure quenching effect can be effected, after it had already become impure for an exact tuning, the purity of the beats seems to play an important part.

c. Also for the quantity of the electro-motive force which is induced in the primary system by
10824 the secondary, the coupling degree is of importance. Under otherwise identical conditions it is proportional to the coupling co-efficient. The greater this is, the greater is the danger that the spark-gap may be again ignited after one half beat.

d. Finally the temperature of electrodes—not the mean temperature, but the maximum temperature at any point of the electrode (local heating)—is of importance for the quenching effect inasmuch as by very high temperature, the gas may be strongly ionized and the quenching effect as well as the discharging voltage may be considerably reduced. This facilitates the re-ignition of the

 10825

spark-gap after the first half beat. Therefore care has to be taken that the temperature does not rise too high at any point of the electrodes.

XQ. 168e. Would you also please translate, Doctor, the two paragraphs of Section 62 of your book, page 111, Defendant's Exhibit D-42? A. In paragraph 59 it has already been stated that under the conditions there considered, the oscillations in the primary and secondary system are similar to those represented in Fig. 130. In the primary system at the end of half a beat, the amplitude of the oscillation is nearly or entirely zero. After that, it is increasing again, because the secondary system whose amplitude at that time is 10826
maximum, induces in the primary system an electro-motive force, and therefore a potential between the electrodes of the spark-gap.

But there may also be the case where the spark-gap, during the time in which the amplitude of the primary system is very small, is de-ionized in such a degree that the electro-motive force induced by the secondary system is no more sufficient to re-ignite the spark-gap. Consequently the spark-gap remains quenched. This effect is therefore called a 'quenching effect,' or 'quenched spark.' The oscillations in the primary circuit then cease entirely and the secondary system oscillates thereafter with its own damping and frequency, as if the primary system did not exist at all." 10827

That is a correct translation from your book, and you so testified before Judge Veeder.

A. I recollect that I have translated a part of my book. Of course, I cannot recollect everything I testified there, but I am absolutely convinced that you have read that correctly.

(Whereupon the court takes a recess until this afternoon at 2:00 o'clock.)

10828 Dr. J. Zenneck—Recalled—Cross.

Afternoon Session 2 o'clock. Continuation of proceedings pursuant to recess. All parties present as at former hearing.

Jonathan Zenneck, same witness, resumes the stand for further cross examination.

Q. (Mr. Betts) Dr. Zenneck, you recognize that Bjerknes' method of measuring the decrement of the quenched spark gap circuit, as a correct method of determining the decrement of the primary circuit of such a transmitter? A. I cannot answer this question with-
10829 out knowing what you call the decrement of a circuit containing a spark gap the amplitude of which is, therefore, not decreasing according to a logarithmic law, but, more or less, linearly.

Q. You said this morning that you testified in the suit before Judge Veeder; in that testimony you considered the Telefunken transmitter which had a quenched spark gap, didn't you? A. I did.

Q. And did you not calculate or determine the decrement of the primary quenched spark gap circuit in that transmitter, using the Bjerknes method? A. I did. I may refer, as to this point, to what I explained on page
10830 15 of my small book, where I stated:

"As this value of the decrement is constant"

—there is an apparent mistake in the translation. It says here,

"is constant for the entire series of oscillations." It ought to be "not constant for the entire series of oscillations."

"It does not properly characterize the decrease in amplitude from cycle to cycle but is the average value of the gradually increasing decrement; its

 10831

> use in practice being very convenient for a qualitive consideration of condenser circuits having a spark gap and corresponding, approximately, to the single and definite decrement, which is a precise and sufficient characterization of the time decrease of the amplitude for condenser circuits having no spark gap."

I further refer to what I explained on page 19:

> "The resonance method is that value of the resistance which, when replacing the spark gap produces the same degree, or rather sharpness, of resonance in the loosely coupled secondary cir- 10832
> cuit."

What you, therefore, measure by means of the resonance method, is nothing but the sharpness of resonance represented by the shape of the resonance curve. You do not measure a decrement which gives you any accurate measurement of the decrease of the amplitude in the primary circuit containing a spark gap.

It is, therefore, impossible to calculate from a decrement measured by this way accurately the decrease of the amplitude and, therefore, the number of oscillations. By making assumptions about the decrease of
amplitude in the primary circuit, such, for instance, that 10833
the decrease is logarithmic or that it is linear, you may get the order of magnitude of the number of oscillations, but you could never get an exact figure for them, and you will never be able to say how far this number of oscillations, calculated under the assumptions made, is correct.

Q. Dr. Zenneck, didn't you, as a matter of fact, use Bjerknes method when you determined the decrement of the primary circuit of the quenched gap transmitter in the suit before Judge Veeder? A. I certainly did.

Q. And you found that decrement——

Mr. Farnsworth: Let him finish his answer.

A. (Continuing) Excuse me. I certainly did, but I think you recollect that you, and I think also Mr. Waterman, insisted on my doing so, and I did it under the assumption that the decrease of amplitude in the primary circuit follows a logarithmic law. I calculated, therefore, what I just said, just the order of magnitude. It is not at all the exact number of the oscillations.

Q. Didn't you testify in that case, in answer to questions propounded to you by Mr. Farnsworth, as follows:

10835 "Q.13. What in general were the measurements you made? A. Oh, I have made measurements about different questions.

"Q.14. Such as what? They were what? You may refer to the memoranda.

"The Court: Why do you not ask him what you want? The Doctor says he has made measurements for various purposes.

"Mr. Farnsworth: Now I am asking him what those various purposes are, your Honor.

"A. I first made measurements about the primary circuit. I measured the amount of damping of this primary circuit. I found in using the well-known Bjerknes method, that the decrement of this primary circuit was between the limits .25
10836 and .35. The first figure I found by using less energy than is generally used; the second figure, that is, .35, I found when using full power."

If you wish I will show you a copy of your testimony.

A. I recollect that. That is correct. It is all right.

Q. And did you not, in the same case, and with respect to the same quenched gap transmitter, calculate how many complete oscillations there were in the primary circuit of that quenched spark gap transmitter before the oscillations were damped down to ten per cent of the maximum, and state that it was 9.2 when this circuit was not coupled to the antenna circuit? A. I repeat, that I did that because you insisted upon my doing it. I did it not in my deposition—I did it in the cross examination and I did it under the assumption, which is expressed in my calculation or was expressly stated, I don't recollect, under the assumption that the decrease of the amplitude in the primary circuit was following the logarithmic law. This assumption shows clearly that I did not intend to do anything else than state the order of the magnitude of the number of oscillations. It would be incorrect to use this assumption for calculating the correct number. 10838

Q. You testified in that case as follows, did you not:

> "XQ. 47. How many complete periods of oscillations in the primary circuit of the Sayville transmitter do your decrement measurements prove to have existed before the oscillations were damped down to 10 per cent of the maximum? A. I have not calculated this.
>
> "XQ. 48. Could you do so? A. Yes, I could do it, but it would take some time. It is very easy to calculate it.
>
> "XQ. 49. Could you do so in a few minutes? A. I can give you the formula and perhaps you could calculate it yourself.
>
> "XQ. 50. I would prefer, Dr. Zenneck, if you would calculate it and state what the result is from your own formula? A. Yes, I can do it. If I have not made a mistake the result is 9.2. That would mean that after nine periods, the amplitude of the current is about one-tenth or 10 per cent of the initial amplitude." 10839

You remember so testifying?

A. I remember that I testified in this way. If you will look at my calculation you will see that I had made the assumption that the amplitude is decreasing according to a logarithmic law. This calculation, therefore, proves very clearly that I only calculated that number under this assumption.

Q. And you remember also testifying in this case as follows, didn't you:

> "XQ. 99. Can you explain the method which you used in measuring the decrement of the antenna circuit? A. I excited the oscillations of the antenna by shock excitation from the primary circuit. I then coupled one of the coils, inserted in the antenna circuit, extremely loosely with my wave meter, in which a hot-wire ammeter, so-called watt meter, was inserted. I took a reso-
> 10841 nance curve, that is to say, I changed the frequency of my wave meter and measured the current for each frequency of this wave meter. Then I plotted this curve and I used the well-known Bjerkness method for calculating the decrement."

You remember so testifying?

A. I remember. I may add in this case where I measured the decrement of the antenna excited by shock excitation method, the formulae and method of Bjerknes can be used and gives correct results, as in this case the decrease of the amplitude in the oscillation of an antenna is practically taking place according to a logar-
10842 ithmic law.

Q. Now, Dr. Zenneck, will you please calculate the number of complete oscillations in the quenched spark primary circuit of the Simpson mercury valve transmitter when it is not coupled to the antenna circuit, using as the basis of your calculation the decrement as found by Mr. Kolster of the Bureau of Standards report (Defendant's Ex. No. 10) as being .45, and using the Bjerknes method or formula which you used in the case before Judge Veeder? A. Using the same assumption which I made there, namely, logarithmic decrease of the amplitude in this circuit, I get the following formula. Shall I

dictate that or just write it down, because these formulae are very hard to dictate. I call the amplitude at the time "t,a"—the amplitude at the beginning, at the starting of oscillations a_0. Then the equation holds: a equals a_0 times e exponent minus delta times=t—t meaning the time—divided by T—T meaning the period. Introducing the number of oscillations, I may substitute for t divided by T, x—x meaning the number of oscillations. We get them by taking the logarithm on both sides of the equation, delta times x equal logarithm ao divided by a, or x, the number of oscillations, equal logarithm a_0
divided by a, this all divided by delta. 10844

Pardon me, did you want me to calculate the number of oscillations?

Q. Down to ten per cent of the maximum. A. Ten per cent?

Q. Down to one per cent of the maximum. A. One per cent?

Q. One per cent. A. Then, if you want me to calculate the number of oscillations after which the amplitude is one per cent of the initial amplitude, that means that a_0 divided by a is 100; therefore the equation is, x equals logarithm 100 divided by the decrement, delta. The natural logarithm of 100 is about 4.6. You have
given me as a basis for my calculations as a decrement, 10845
as I understood, .45, is that correct?

Q. That is correct. A. Therefore the number of oscillations is 4.6 divided by .45. This gives 10.2. It is therefore about 10. This number means that after 10.2 complete oscillations the amplitude is one per cent. of the initial amplitude. I may add to that, that I have calculated this number here under the assumption of a logarithmic decrease of the amplitude of the circuit. I may add further that the exact number of oscillations cannot be determined under any assumption, owing to what I have just referred to and what I have explained on pages 15 and 16 of my book, that the decrement measured by the Bjerknes method for a circuit the decrease of amplitude of which does not follow a logarithmic law, does not properly characterize the decrease in amplitude from cycle to cycle.

Q. Now, will you please calculate the number of complete oscillations in the primary quenched gap circuit of the Simpson mercury valve transmitter, assuming that that decrement is .45, as found in Mr. Kolster's Bureau of Standard's report, using the linear formula of de-
10847 crement, to which you have referred? A. I cannot do that.

Q. I only ask you for the result; I do not care for you to put into the record the exact formula which you use—but you can if you wish. A. Mr. Betts, I cannot do that. The measurement of the decrement, or I should say a decrement measured by Bjerknes method for a circuit the decrease of the amplitude of which is linear and not logarithmic, does not give you any indication how to calcalculate the number of oscillations in the primary circuit. That is impossible.

Q. Is there not a formula for calculating the linear decrement? A. No, there is no formula. May I explain
10848 that. I know that a formula has been used, which is given in my small book on page 68, equation No. 5, giving the effective current for a linearly damped oscillation; the "a" in this equation means the linear decrement. Now, this formula has been compared with the formula 4 given on the same page containing the logarithmic decrement d, and it has, therefore, been said that [comparing these formulas], that six times the linear decrement corresponds to four times the logarithmic decrement. But those two formulas have nothing whatsoever to do with the Bjerknes method and the use of these two formulae

 10849

for the purpose of calculating the number of oscillations by a decrement which has been measured by Bjerknes method, would be an apparent misuse.

Q. Then, Dr. Zenneck, you do not believe in the Taege formula as being absolutely correct? A. I certainly believe in the Taege formula, as Taege was one of my students and has made his research work under my supervision. But Taege's formulae apply to the following case. He has a primary circuit and assumes that the decrease of the amplitude of its oscillations follow the linear law. He further assumes that this primary circuit is extremely
loosely coupled with a secondary circuit, and now he cal- 10850
culates the effective current of the secondary circuit. But Taege has not calculated the resonance curve, which you would get in this case, and by which you, [perhaps, if you had it], could calculate this linear decrement. Therefore, it is impossible to deduce anything about the matter in question from Taege's formula. I may add that Taege and I have tried for several months to get the resonance curve for linearly damped primary oscillations, but have not succeeded.

Q. Using Taege's work and assuming that the decrement of a circle is .45, can you tell me the number of
oscillations occurring in that circuit, assuming the linear 10851
decrement? A. No, I cannot.

Q. Why not; is it impossible? A. It is impossible.

Q. Don't you think that anybody can do it? A. I do not think they can. The first condition for that is to have the equation of the resonance curve for a primary circuit in which the decrease of the amplitude follows a linear law, and, as I told you, we have not succeeded in getting this formula, and nobody else has.

Q. Dr. Zenneck, from your experience in the use of and your observation of wireless transmitters, it is cor-

rect, is it not, to say that the use of tight coupling, as distinguished from loose coupling, is advantageous, because, with the former adjustment the transmitter will radiate more energy than in the latter? A. In this form it is not correct. Tight coupling means a close reaction between the primary and the secondary circuit. Now, suppose an ordinary coupled transmitter containing, for instance, a zinc spark gap of the length of at least half a centimeter. Here when you make the coupling very tight, the amplitude in the secondary circuit cannot be increased over certain limits by increasing the coupling, because the
10853 tight coupling at the same time means a strong retransfer of energy from the secondary to the primary circuit. Furthermore you get two oscillations of the antenna and as far as I know in all receivers only one is made use of. By reason of this fact the useful, so to say, radiation is still more decreased.

If you consider a quenched gap transmitter where, after the first half beat, that is to say, after the energy had been transferred to the secondary, the gap in the primary is quenched, and therefore, the primary circuit practically disconnected, there cannot be any longer any transfer of energy from the secondary to the primary.
10854 Now the length of a beat, therefore the length of time after which this disconnecting happens, is determined by the coupling, and is the shorter the tighter the coupling is. In this respect, therefore it is certainly favorable to have a tight coupling and, therefore, to have the primary circuit oscillating during as a short time as possible. But even in this case the question is somewhat complicated by the fact that those quenched gaps which allow a very tight coupling have mostly also high energy consumption. Even in this case, therefore, it is doubtful whether the energy absorbed in the primary spark gap

is greater with tight coupling or with a set permitting tight coupling, than with a set permitting a less tight coupling. But it is generally considered favorable to have a quenched gap which allows a very tight coupling.

Q. Then, as I understand you, Dr. Zenneck, when a transmitter is too tightly coupled, beats result—when it is too tightly coupled it produces beats? A. That can be the case, certainly, but——

Q. And——

Mr. Skeel: Let him finish his answer.

Mr. Betts: Certainly—if you will let me know when you finish. 10856

A. Pardon me. That can be the case for a definite certain spark gap. As a matter of fact, if you use a quenched gap as they are mostly used in practice, you can increase the coupling to a certain limit, and if you go beyond this limit there may be at least beats resulting.

Q. Do I understand you that you cannot answer my last question yes or no?

A. (Continuing) I understand too tightly coupled, as meaning that the coupling is tighter than the tightest coupling which can [be used, or which is used in practice, or can] be used in practice, in order to get good quenching effect. Thus, if the coupling is made still tighter, then, as far as I know, or according to my experience, you get beats. 10857

Q. Could you draw a diagram showing what you mean happens when too tight a coupling is employed in the transmitter?

Mr. Farnsworth: Do you mean with the quenched gap or not, Mr. Betts?

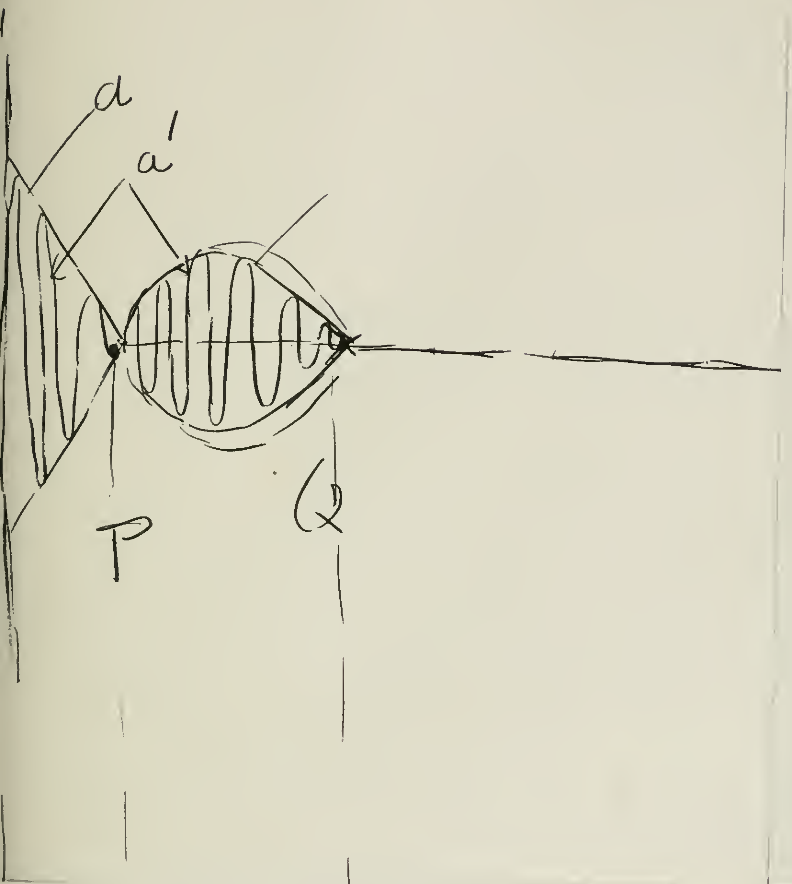
Mr. Betts: I mean with the quenched gap.

A. Yes, I can do that; of course only in a very diagrammatic way. I will show the oscillations in the primary circuit and the secondary on Z.15 (draws diagram). (Reproduced opposite.)

The Witness: I have first drawn the amplitude curves, as marked Fig. 1; the curves a in Fig. 1 and b in Fig. 2. I will now show in a very diagrammatic way the oscillations. The oscillation curve marked a^1 in the Fig. 1 and b^1 in Fig. 2. Of course I have to state that [of course] the period of the oscillations should correspond in
10859 these two curves, but this is hardly possible without squared paper.

Q. You mean the curves in Figs. 1 and 2? A. The curves in Figs. 1 and 2. This curve shows the following, when the coupling is tighter than the quenched spark gap allows, and we do not get the quenching effect after the first half beat, that is to say, at the time, which I will mark in the primary circuit of Fig. 1 as p. But the quenched gap in the primary circuit is again re-ignited [it is] and we get the phenomenon of beats. Then after one and one-half beats, that is to say, at the time
10860 which I mark now "q," the quenching may occur. We get then from this time on—well, I will mark it q and also on figure 2—the free oscillations of the secondary circuit, the primary circuit being electrically disconnected. I refer in this respect to a figure in my small book showing a similar condition, the figure 133 on page 96. In this figure photographs had been made of a spark gap in the primary and also of a very small spark gap which had been inserted into the secondary circuit. This figure shows two and one-half beats in the primary and the corresponding quenching effect only after two and one-half beats.

Z 15



Z 15

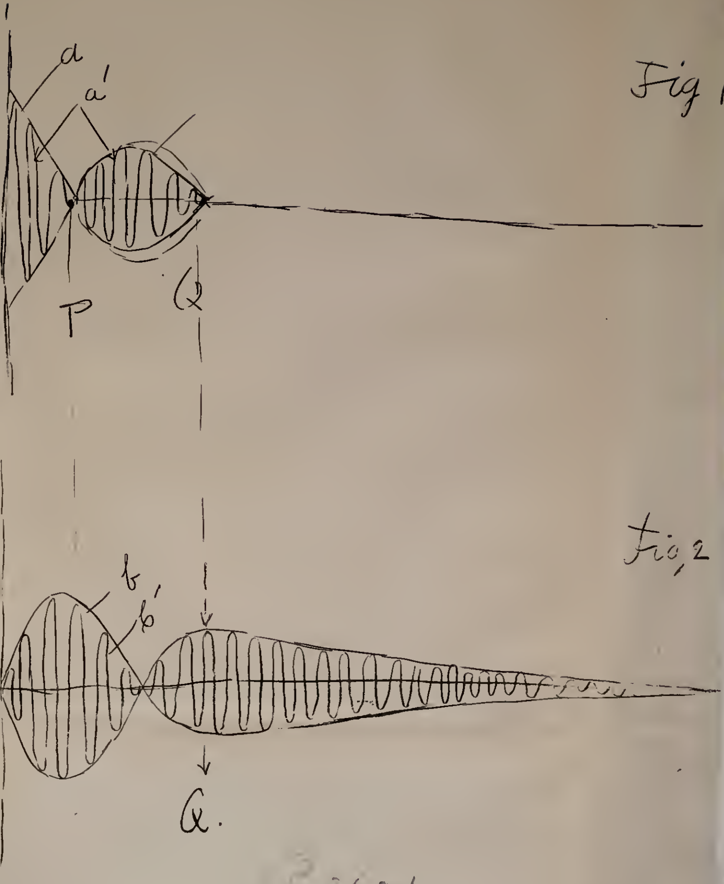


Fig 1

Fig 2

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Q. (Mr. Betts) The drawing which you have just made, Dr. Zenneck, Z-15, shows one and one-half beats in the primary circuit? A. Yes. I may add that by making the coupling still tighter for the same spark gap you may get two and one-half in the primary, or three and one-half and so on.

Q. And your drawing, Z-15, would result, if the quenched spark transmitter was adjusted so as to produce these beats in the radiation of an impure wave, or two frequencies of coupled waves? A. No. It would result in the radiation of three waves; namely, two during the time during which these beats are present and 10868
one after the primary circuit has been quenched—when, therefore, the free oscillations of the secondary are present.

Q. You have referred this morning, Doctor Zenneck, to a coupled circuit transmitter; can you explain what you meant by that phrase, or those words? A. I understood a coupled circuit transmitter in the connection I used it as a transmitter consisting of a primary, condenser circuit and of a secondary antenna, and containing further in the primary a spark gap without quenching action.

Q. Is it not a fact that in such a coupled circuit transmitter as you have just described, that the coupling can 10869
be adjusted so that this transmitter will radiate only a single pure wave? A. It can be adjusted so, if the coupling is not made tight enough as to give two so-called coupling waves, and if the primary and the secondary circuit have the same frequency.

Q. As I understand your last answer, you mean that if the circuits are coupled too tight, beats will occur? A. Certainly.

Q. It is true, is it not, that the decrement or rate of decay in the primary circuit of a coupled circuit trans-

10870 mitter, as you have described it, may be the same as when a quenched gap is used? A. In this case you cannot talk about a decrement of the waves emitted. As I already stated, the condition of the definite decrement is a logarithmic decrease of the amplitude. Now the condition for a definite decrement is that the amplitude curve is an exponential curve, and this is not the case in a coupled transmitter. The character of the amplitude curve of such a transmitter is shown in the chart which I shall mark "Z-16." In such a case you first get a gradual increase of the amplitude, and then a gradual de-
10871 crease of it. The gradual decrease generally not corresponding to exponential law.

Q. I think, Doctor, you misunderstood my question. I was referring to the primary circuit, while, I think, you are referring to the antenna circuit, are you not? A. I am referring to the antenna circuit.

Q. My question was as to the primary circuit—a spark gap circuit. A. Shall I have the question again.

Question repeated to the witness.

Mr. Farnsworth: In Z-16, is not the primary circuit shown there too?

The Witness: No. That means the secondary
10872 circuit that I have shown. I beg pardon, I misunderstood the question.

The Court: Then do you want to withdraw this exhibit too?

Mr. Betts: I think he misunderstood the question.

The Witness: I misunderstood the question. Cross that all out.

The Court: Then you can cross all that out.

The Witness: You mean by that, that the decrement of the primary circuit alone may be the same sort of decrement of waves?

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Q. No, Doctor; I do not quite mean that; I will put the question again: I ask if it is not true that the rate of decay, or the decrement, as it is called, in the primary circuit of a coupled transmitter, as you have described it, may not be the same in the case of the quenched gap transmitter—the primary of the quenched gap transmitter; in other words, that the rate of decay of the two transmitters might be the same? A. In both cases, the primary circuit considered alone?

Q. Yes. A. Therefore, if I understand your question correctly, you want me to compare the rate of decay or decrement of a primary circuit such as is used in a coupled 10874
circuit transmitter with the rate of decay or decrement in the primary circuit such as is used in a quenched gap transmitter; is that right?

Q. Yes, that is right, and I say they may be the same in both cases? A. No, it may not be the same.

Q. Never? A. No. Not in a practical case. Because in a quenched gap transmitter spark gaps are used which have a high de-ionizing power in order to give the quenching, and these gaps have, in general, a high spark gap resistance. Whilst in the coupled transmitter spark gaps are used which do not quench and which generally have by far similar so-called resistance. Therefore, the oscil- 10875
lations in the primary of a coupled circuit transmitter are, generally by far less damped than those in the quenched gap transmitter.

Q. Have you finished? A. I have finished, yes.

Q. Now, consider the case where in the two transmitters, you have the primary circuits coupled to the antenna circuits; would the rate of decay or decrement in the first half beat of the primary circuit of the coupled transmitter be any differefnt from that of the quenched gap transmitter? A. What is the question again, please?

(Question repeated to the witness.)

10876 Dr. J. Zenneck—Recalled—Cross.

A. The rate of decay during the first half beat is practically defined by the interference between the two coupling waves, and the frequency of those two coupling waves is determined by the coupling degree. Therefore, this rate of decay is not at all, or very little affected by the properties of the spark gap. It may be, therefore, the same in a coupled circuit transmitter as in a quenched gap transmitter. The only thing which may happen in a quenched gap transmitter is that the gap may be quenched before the amplitude of the resulting oscillations becomes zero.

10877 Q. Now, referring, Dr. Zenneck, to page 94 of your small book, to the figures on that page; as I understand it, the top figure shows the beats or oscillations in a primary circuit of a coupled circuit transmitter? A. Yes.

Q. And the third figure from the top on that page shows the beats of oscillations in the primary circuit of a quenched gap transmitter? A. Certainly.

Q. And there are the same number of oscillations in both half beats? A. In both half beats, yes. I may say that these figures, are, of course, diagrammatic. I made them exactly equal, as you will see, if you compare them. They do not, therefore, refer to any practical arrangement—they are just diagrammatical. But, as I just
10878 stated in my answer before, also in a practical arrangement at least practically the same would happen, supposing that you have the same coupling in both cases, with the coupled transmitter and with the quenched gap transmitter.

Q. I believe you referred a moment ago to the coupled circuit transmitters as characterized by some special form of spark gap, namely, one that did not quench; am I correct? A. Yes, that is the difference, or one of the differences between the coupled circuit transmitter and the quenched spark gap transmitter.

Q. Could you describe the constructions of those spark gaps which could be used in the coupled circuit transmitter? A. I can refer to page 181 of my small book, Fig. 217, which shows the spark gap which had been used by the Telefunken Company for coupled circuit transmitter.

Q. What is the peculiarity of the spark gap illustrated on the page of your book which you have just referred to, that makes the transmitter a coupled circuit transmitter? A. In consequence of the rather large distance between the electrodes, this gap has no quenching
effect. The ionization in such a gap persists for a rela- 10980
tively very long time.

Q. Suppose several of the gaps illustrated in Fig. 217 on your small book, were connected in series; would the transmitter still be a coupled circuit transmitter? A. A gap like that illustrated here would, as far as my experience goes, not make a practical quenched gap, even if you connected, say, five or six in series.

Q. How can you say if the spacing between the electrodes in the spark gap illustrated on page 181 of your book, were made a little closer together and several of them connected in series; would that transmitter then be
a coupled circuit transmitter? A. If you made the dis- 10981
tance of the electrodes here, say, one-tenth or two-tenths of a millimeter and used as the material of the gaps copper or silver, and if you connected say, six or eight in series, this would give a spark gap which would have a quenching effect, although not being very appropriate for this use.

Q. At what distance or separation of the plates of the spark gap illustrated in Fig. 217 on page 181 of your small book, would the transmitter cease to be a coupled circuit transmitter and become a quenched gap transmitter? A. Why, you cannot give a line of demarcation. If you say the distance is, say, one centimeter, it would certainly not have any appreciable quenching effect. On the other hand, if the distance is about, as I said, one-tenth or two-tenths of a millimeter and you have more of them in series, it is, or may be a tolerable quenched gap.

In the intermediate case, say, where the distance between the electrodes be three or four or five-tenths of a millimeter, and if, say, only two or only one of them were used, you would get such intermediate effects as I have
10883 shown on chart Z-15.

Q. In what figure, Dr. Zenneck? A. In both of them, the primary and the secondary.

Q. If the separation of the electrodes of the spark gap illustrated in 217, or such a spark as illustrated in 217 of your small book, was a little too wide, as I understand your last answer, the coupling would have to be looser? A. For what purpose?

Q. To avoid what you have called the coupled circuit transmitter action, that is to avoid the formation of beats, such as you have shown on your drawing Z-15. A. If in this case the coupling is made loose enough you might get
10884 a quenching after the first half beat. But the duration of this beat being in this case very long, this would mean that in the secondary circuit there are, for a relatively long time the two coupling waves present, and only, after a relatively long time, the free oscillations of this secondary system would start.

Q. Is it true that the looser the coupling the longer would be the first half beat in any closed spark gap transmitter with which you are acquainted? A. Certainly; always supposing the same spark gap.

(Whereupon a short recess is taken, and after recess the hearing proceeds, as follows:)

Q. (Mr. Betts) If I correctly understand you, Dr. Zenneck, it is your view that there might have been better ways of arranging the details of the connection to the Braun tube during the Dr. Chaffee tests at the Cruft High Tension Laboratory of the Simpson mercury valve transmitter, but that you agree that the result of those tests as a whole is to show that there were at least two and one half complete oscillations in the primary circuit of that transmitter, and that this is shown by the tests 10886 made both with the primary circuit and upon the secondary or antenna circuit. A. I expressed my opinion that Dr. Chaffee should have avoided in his arrangements some questionable points which I have mentioned, in order to make his tests conclusive. But it may be that there have been really oscillations in the so-called primary circuit, whether just exactly two and one half or three, I do not know. I have assumed in the last part of my testimony that a number of oscillations about like that were present, not laying much stress on the exact number of these oscillations.

Q. Then you do agree that taking Dr. Chaffee's tests as a whole they showed that there were at least two and 10887 one half, or possibly three complete oscillations in the spark gap circuit of the Simpson mercury valve transmitter.

Mr. Farnsworth: You mean that particular Simpson mercury valve transmitter under test?

Mr. Betts: I do.

A. I agree that there have been some oscillations, as I could not explain the result of the tests in another way. But in view of the points which I have disapproved, I do not know whether two and one half is just the number of these oscillations.

Q. Will you please now consider the circuit shown in your sketch Z-1, which, as I understand you, represents the Simpson mercury valve transmitter, and suppose that a single open spark gap, equivalent to the five gaps used, was substituted for the five gaps, would that transmitter be then a coupled circuit transmitter? A. If you substitute for this gap G a spark gap such as used in the coupled circuit transmitter you would generally get two oscillations. But as I have explained with reference to
10889 some work of Wien and Slaby, on Note 91, page 415, of my small book, under certain conditions such a transmitter could also give one oscillation. Therefore, this transmitter, owing to its different initial conditions, is not identical with a magnetically coupled transmitter—even if there is an ordinary plain spark gap.

Q. Considering the circuits shown in your diagram Z-1, which you say represents the Simpson mercury valve transmitter, and suppose that a single open spark gap was substituted for the five gaps, would then, in your opinion, this transmitter be an impulse transmitter? A. An impulse transmitter?

Q. Yes, leaving everything else the same. A. I have
10890 no experience with this transmitter, but I do not think that it would be an impulse transmitter, because I do not think that in this case the result would be the free oscillations of the circuit C^1, L, S, C.

Q. You have, I believe, testified that the spark gap circuit of the Simpson mercury valve transmitter does not persistently oscillate? A. No.

Q. This is because it has a spark gap in it? A. No, this is because it has a small number of oscillations.

Q. You have referred to the fact that Dr. Chaffee obtained beats when the spark gap became warm. Was

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energy being transferred back from the antenna circuit to the primary circuit when such beats occurred, as shown in your diagram Z-15? A. The presence of beats shows only the existence of two oscillations of different frequency and, therefore, I conclude that when Dr. Chaffee has got beats he had two different frequencies in his set. If Dr. Chaffee had got these beats in the transmitter shown on chart Z-4, I would certainly say that these beats indicate the retransfer of energy from this circuit C^1, L, R^1, P^1, C^{11}, to the circuit C, PQ, L^1, G. In the case of chart Z-1 I would not draw this conclusion, because the condenser C and also the inductance PQ is 10992
common to both circuits.

Q. When you made reference to Dr. Chaffee having obtained beats when the Simpson mercury valve transmitter was tested and the spark gap became warm, energy was being transferred back and forth between the spark gap circuit and the antenna circuit, as you have indicated in Z-15? A. No, I just said that I would not draw this conclusion, owing to the fact that the condenser C and the inductance PQ are common to both circuits.

Q. Well, at the time Dr. Chaffee conducted the tests, and beats were indicated, energy was being transferred between two circuits? A. I do not see from which of the results of Chaffee you draw this conclusion. 10993

Q. What was happening when beats occurred when Dr. Chaffee made his tests in your presence on the Simpson mercury valve transmitter? A. Assuming that his arrangement was all right, as measuring the current in the lead connecting the spark gap, the presence of beats in this lead, therefore, as I said, does not prove anything else but the existence of two oscillations of two different frequencies in this lead. I may further say that this current here does not give you any indication of the energy situation in the whole set.

10894 Dr. J. Zenneck—Recalled—Cross.

Q. Isn't it true that oscillations of two different frequencies can only occur when there are two circuits coupled together? A. Two oscillations of two different frequencies occur or may occur if there are two different equations defining the operation of the set, and this shows that there is some influence between two currents. This may be expressed by saying that there are two coupled circuits, but this more or less indefinite expression means then nothing more than what I just expressed, the mutual influencing of two currents or charges.

Q. What two circuits did those two currents flow in
10895 that you have just mentioned? A. In the direct testimony I have already stated that you may say that one current is flowing in the circuit C, PQ, L^1, G, and the other flowing in the circuit C^1, L, S, C, of chart Z-1. Or, you may just as well assume that according to chart Z-2 one current is flowing in the circuit C, PQ, L^1, G, and the other in the circuit C^1, L, RQ, L^1, G. Both of these statements give the same result.

Q. When you find two frequencies in a single circuit it is satisfactory evidence to you, as a scientist, that such a circuit is coupled or influenced by another circuit also having oscillations, is that true? A. You mean two oscil-
10896 lations at the same time?

Q. Two frequencies in a single circuit? A. Yes, at the same time?

Q. At the same time. A. I do not know any other example where this happens except when the first circuit is influenced, or when there is a connection between this circuit and another oscillating circuit.

Q. Then you answer my question "Yes". A. What is that?

Q. Then your answer to my question is in the affirmative? A. Yes.

 10897

Q. Now, referring to Dr. Chaffee's photograph No. 53, which I show you, that is the way you explain the existence of beats in that photograph? A. Yes, this is a photograph showing these beats.

Q. Assuming that the Simpson mercury valve transmitter set tested by Dr. Chaffee on July 3 and 4 was operating with perfect regularity and constant current of a given amplitude in the spark gap circuit, can you make a diagram showing the image on the screen of the Braun tube when the spot was deflected by the current of the spark gap circuit? A. I do not understand what you mean by constant amplitude, you mean undamped oscilla- 10898
tions?

Q. No, constant current of a given amplitude, constant current. A. That must be a mistake, Mr. Betts.

Q. What do you mean by free oscillations in a transmitting wireless circuit? A. I mean by free oscillations of circuit the oscillations, the damping and frequency of which is determined by the constants of the circuit.

Q. You have referred to coupled circuit transmitters, can you refer to a diagram in your small book showing the wireless circuits of such transmitters? A. Wireless circuits, you mean the circuit connections?

Q. The oscillating circuits of such a transmitter. A. 10899
The oscillations of such a transmitter?

Q. The circuits, the oscillating circuits of such a transmitter. A. I understand your question as meaning the circuit connections of such a transmitter.

Q. That is correct, Doctor. A. And I therefore refer to page 175 in my small book, on which the figures 210 to 213 represent such circuit arrangements.

Q. Assume that you insert a given spark gap into a closed circuit containing good condensers and inductance coils of low resistance, such as in the Simpson mercury

10900 valve transmitter, and also insert a like spark gap into an antenna circuit having a resistance (radiation and ohmic) of four ohms; which circuit will have the greater number of oscillations in it? A. In the first case, consisting of a condenser circuit having no appreciable resistance, the number of oscillations is, as I explained, entirely determined by the spark gap. The only place where the energy is consumed is the spark gap.

In the second case, the antenna, energy is not only consumed in the spark gap but also in the ohmic resistance of four ohms, and also by radiation. The so-called
10901 spark resistance in the second case would also not be the same as in the first. I have stated on page 17 of my small book: "The gap resistance and decrement are however not independent of the resistance circuit, both increasing for an increase of the circuit resistance."

The result, therefore, is that in the second case there would be certainly a smaller number of oscillations than in the first case.

Q. That is, there would be a smaller number of oscillations in the antenna circuit? A. Certainly.

Q. And to what is that difference due, to radiation? A. This difference is due, first, to radiation; second, to the resistance of four ohms, which you assumed to be
10902 inserted into the circuit, and, third, to the increase of the spark resistance, to which I just referred.

Q. Four ohms is not a large resistance for an ordinary ship's antenna, is it, radiation resistance for an ordinary ship's antenna? A. I do not know what is the radiation resistance of an ordinary ship's antenna. I only know that in the report of the Committee on Standardization, to which I have referred before, four ohms is given as the total resistance of a ship's antenna for 600 meters, and a capacity of about .001 micro-farads.

I understood your question before as meaning ohmic resistance of four ohms is inserted in the antenna, is that right, that was in the question before?

Q. No, my question, Dr. Zenneck, said that the total resistance of the antenna, ohmic and radiation resistance, was assumed to be four ohms. A. You mean that in the question before?

Q. Yes. A. Then, of course, my answer has to be corrected. I understood that you meant radiation resistance and a resistance of four ohms.

Q. Now, answer the question, understanding that I mean that the total resistance of the antenna, both ohmic and radiation resistance, are together four ohms. A. 10904
The result is the same. As I said, the number of oscillations in the second case, the antenna case, would be smaller than in the case of the condenser circuit, owing to the fact that in the second case, first, the consumption of energy by radiation and in the ohmic resistance is added, and, second, because the spark resistance in the second case would be greater than in the first, for the reason I stated before.

Q. Now, you have referred in your direct examination to Dr. Chaffee's plate BM. Will you please explain what you understand that plate to show? A. It seems to show, as I already stated in my direct examination, 10905
that the amplitude of the current in the dummy antenna to which this figure refers is increasing during two and one half oscillations, and then gradually decreasing.

Q. Assuming that Dr. Chaffee's tests of the Simpson mercury valve transmitter showed that there were six complete oscillations in the spark gap circuit when it was not associated with the antenna circuit, and that there were two and one half oscillations in the spark gap circuit when it was associated with the antenna circuit, will you please explain what caused this difference? A. The difference is that in the first case when the condenser circuit was not associated with an antenna the decrease of the amplitude was caused by the constants of the circuit. In the second case, where we had only two and one half oscillations, the decrease of amplitude was caused, as I have explained, by the interference of two oscillations.

Q. What do you mean by the constants of the circuit, in your last answer? A. I mean by the constants of the circuit the capacity, the inductance, the ohmic resistance and the so-called spark resistance.

Q. Now, the transfer of energy from the spark gap
10907 circuit to the antenna circuit in the Simpson mercury valve transmitter is equivalent to a constant of the resistance added to the spark gap circuit, so far as the number of oscillations is concerned, is it not? A. That seems to be an arbitrary interpretation of this fact. There is no question that an increase of resistance would cause a decrease of the number of oscillations, but there is no reason to say that whatever the number of oscillations is, it is equivalent to an increase of resistance.

Q. Can you calculate or give me the formula of the resistance of the spark gap in ohms? A. No.

Q. Is it not true, Dr. Zenneck, that in any transmitter the number of oscillations in the spark gap circuit
10908 will be less when the spark gap circuit is coupled to the antenna circuit than when the spark gap circuit is not coupled to the antenna circuit? A. It is not possible to answer the question in this form, as the form of the amplitude curve in both circuits may be so different, owing, for instance, to the presence of beats, that it is hardly possible to compare such two circuits. The number of oscillations, for instance, in a circuit in which the decrease of the amplitude is following a logarithmic law, is, of course, conventional. I think it has been agreed here to say that the number of oscillations is defined by the am-

 10909

plitude being down to one per cent of the initial amplitude. Now, as in a circuit having beats, such as I have represented on page 94, Fig. 130 of my small book, the amplitude of the oscillations comes down to zero several times, the question is not definite.

Q. I think, Doctor, that you may have misunderstood my question, and so I will re-state it. A. Yes.

Q. Isn't it a fact that in any transmitter the number of oscillations in the spark gap circuit will be less when that circuit is coupled to the antenna circuit than when that same circuit is not coupled to the antenna circuit?
A. I did not consider that you said that there was a spark 10910
gap in the primary. I would say that if a condenser circuit containing a spark gap is coupled with a secondary circuit then the number of oscillations after which the oscillations in the primary circuit cease is smaller than in the case when the circuit is not coupled to another circuit.

Q. As I understand your diagrams which you produced in your direct examination, you are of the opinion that in the Simpson mercury valve transmitter the primary spark gap circuit may be said not only to build up oscillations in the antenna circuit, but to thereafter maintain them for two and one half or three oscillations in
the primary circuit. 10911

Mr. Farnsworth: Is there two parts to that question, Mr. Betts?

Mr. Betts: Two parts, no, it is all one question.

(Question repeated to witness.)

A. That is not my opinion. I have explained already, referring to chart Z-1, that initially the charge is contained in the condenser C and the condenser C^1. This condenser C^1 is common to this circuit and to this circuit (Indicating). Initially the energy is contained in the electric field of this condenser and in the electric field of this condenser.

Mr. Farnsworth: That is, C and C^1.

A. C and C^1. As this condenser C is common to both circuits there is no reason whatever to say that the energy is contained in this primary circuit. Now, the result consists in free oscillations of the system C^1, L^1, S, C. The result, therefore, is that the energy which had been first concentrated in the condensers C and C^1, is now contained in the circuit C^1, L, S, C, in the form of
10913 oscillations. I do not see any reason for saying that there has been a transfer of energy from the so-called primary circuit to the so-called secondary circuit. I sum up again; at the beginning the energy was in the condenser C and in the condenser C^1. At the end, and this is as soon as the free oscillations start, it is contained in the circuit C^1, L, S, C, and if we consider the moment where the current is zero in this circuit, it is again contained in the condensers C^1 and C. I do not see any reason which would justify calling that transfer of energy from the so-called primary to the so-called secondary circuit. or why that should be called building up of those so-called secondary oscillations by the primary oscillations.
10914 The fact is that at first these two condensers were charged, and at last we had oscillations in the condenser circuit C^1, C. Therefore, the energy contained in it has been transformed into oscillations.

(Whereupon the court takes an adjournment to Wednesday, July 26th, 1916, at the hour of 9:30 o'clock a. m.)

 10915

Wednesday, July 26, 1916, 9:30 o'clock a. m. Continuation of proceedings pursuant to adjournment. All parties present as at former hearing.

Jonathan Zenneck, same witness, resumes the stand for further cross examination.

Q. (Mr. Betts) As I understand you, Dr. Zenneck, your diagram Z-7 shows the operation of the antenna of the Simpson mercury valve transmitter when the decrement of that antenna is .06? A. Yes, that is right.

Q. And your diagram Z-8 shows the operation of the 10916
antenna of the Simpson mercury valve transmitter when the decrement of that antenna is .04? A. That is right. I have made these diagrams according to the result which Dr. Chaffee received showing the increase of the amplitude during two and one-half oscillations.

Q. And your diagram Z-14 shows the operation of the spark gap circuit of the Simpson mercury valve transmitter—— A. It shows——

Q. Wait a moment—having two and one-half oscillations? A. It shows two and one-half oscillations, according to the statement of Dr. Chaffee.

Q. Now, referring to your diagram Z-7; on what part 10917
of that diagram, or at what oscillation, does the antenna begin to radiate? A. The antenna begins to radiate as soon as there are oscillations in it.

Q. Well, could you indicate? A. From the beginning.

Q. Could you indicate at what point? A. What part?

Q. At what point on the diagram. A. The point "O" or zero.

Q. And that is equally true with respect to the diagram Z-8? A. Of course.

Q. Now, as I understand your diagram Z-7, the oscillations in the antenna of the Simpson mercury valve transmitter build up until they reach a maximum, at how many oscillations? A. About two and one-half.

Q. And that is equally true with respect to your diagram Z-8? A. Of course.

Q. And after two and one-half oscillations in the antenna of the Simpson mercury valve transmitter, as shown in your diagrams Z-7 and Z-8, the oscillations in the antenna no longer build up; they decay? A. No, they decay.

Q. And in your diagram Z-14 you have intended to
10919 show that after two and one-half oscillations in the spark gap circuit the oscillations are quenched, due to the action of the spark gap? A. They are quenched, yes.

Q. In your diagram Z-1 to Z-5—those were the ones which you drew, you remember? A. Yes.

Q. The condenser c^1 was the condenser which was intended to represent the capacity, the equivalent capacity of an ordinary commercial antenna? A. Yes.

Q. Then that capacity, or condenser c^1, in your diagrams Z-1 to Z-5, inclusive, corresponds with the capacity of the antenna a f in plaintiff's exhibit No. 68? A. It does, of course the difference is that there is a distributed capacity, and in the case of the Dr. Chaffee
10920 experiments there was what we call a lumped capacity, I mean concentrated capacity—concentrated in this Leyden jar, in this condenser.

Q. That is because Dr. Chaffee used a dummy antenna instead of the regular antenna? A. Yes.

Q. And the condenser C in your diagram corresponds with the condenser c, dd^1 in plaintiff's exhibit No. 68? A. Yes.

Q. And the spark gap g in your diagram Z-1 corresponds with the spark gap g in plaintiff's exhibit No. 68? A. Yes.

 10921

Q. And the inductance 2a in plaintiff's exhibit No. 68 corresponds with what, in your diagram Z-1? A. L^1.

Q And the inductance 2 dd^1 in plaintiff's exhibit 68 corresponds with what in your diagram Z-1? A. The whole inductance here corresponds to what I marked in my diagram S or PR.

Q. The inductance g in plaintiff's exhibit No. 68 corresponds—— A. With my inductance L in Z-1.

Mr. Farnsworth: In Z-1?

A. That is right.

Q. (Mr. Betts) Referring to the formula on page 13 10922
of your small book, defendant's exhibit No. 62, will you please tell me the decrement of a circuit having a resistance of one ohm, a capacity of .07 microfarads and an inductance of 1.6 micro-henrys, and having no spark gap? A. A resistance of one ohm?

Q. Yes. A. And a capacity of——

Q. .07 microfarads. A. .07 microfarads?

Q. And an inductance of .06 micro-henrys? A. 1.6 micro-henrys.

Q. And no spark gap. A. I find, if I do not mistake, .07.

Q. And that would represent about eight oscillations? 10923
A. That would represent about 6.4 oscillations, understanding that after the time of 6.4 oscillations the amplitude is one per cent. of the initial amplitude.

Q. Now, if the spark gap was put into that circuit, this would not increase the number of oscillations, would it? A. No. This would certainly not increase the number of oscillations.

Q. Then the ratio of C over L, or capacity to inductance, does have something to do with the number of oscillations, because it fixes the maximum which cannot be exceeded. A. Because it fixes the maximum——

Q. ——which cannot be increased. A. If you arbitrarily fix a certain resistance, what you really did, then the limit which you can get—there no other energy consumption—is fixed by the ratio being of C over L—the limit, nothing else.

Q. The maximum limit? A. The maximum limit. As you ask, I may add the following: You have here fixed a resistance of one ohm in such a circuit. This resistance for such a circuit is relatively very high. If you, therefore, put into the circuit now a spark gap, as you are supposing in your question, a relatively great part
10925 of the energy consumption would take place in this ohmic resistance which is supposed to be inserted. We would, therefore, not have the case where the main energy consumption takes place in the spark gap. When I said that this formula on page 13 has nothing to do with a condenser circuit containing a spark gap, I referred to the circuit used by Dr. Chaffee, where the amplitude curve, which I have represented in diagram Z-12, shows that the main energy consumption had taken place in the spark gap. That is the situation.

Q. Let me ask you if you agree with Mr. Kolster's statement in the Bureau of Standards report, defendant's exhibit No. 10, beginning at page 5, line 19, as fol-
10926 lows:

> "On sheet 3 I have given the result obtained when the special type of spark gap was replaced by an ordinary plain, open gap and without the use of the mercury valve rectifier. The resonance curve indicates the existence in the system of two wave lengths in addition to the free or natural wave lengths of the antenna circuit. This behavior is characteristic of a system with two degrees of freedom, when no attempt is made to suppress the coupling waves. In this case the free or natural wave length of the antenna circuit ap-

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pears rather more prominently than is usual. This is largely due to the fact that even without the use of a special type of spark gap, the oscillations in the trigger circuit are comparatively highly damped, because of the small value of L over C."

A. I do not agree with the statement. I have explained my reasons in the direct examination, and I think that the Bureau of Standards has overlooked the difference which exists in this point between a condenser circuit containing a spark gap and that without the spark
gap, just as Mr. Waterman did. 10928

Q. Assuming that the antenna a in plaintiff's exhibit No. 68 has a capacity of .001 microfarads before the condenser C is inserted. Would the capacity of the antenna be affected by inserting the condenser C if that condenser has a capacity of .07 microfarads? A. Please read that again.

(Question repeated.)

A. Please, what do you mean by the capacity of an antenna in this connection?

Q. I mean the distributed capacity? A. The distrib-
uted capacity, that is to say the capacity of the upper 10929
part here would very likely not be very materially affected, in view of the or relatively high capacity of the condenser C. It would be somewhat affected by the fact that by inserting the condenser C the frequency of the antenna would be somewhat changed, and therefore also the distributed capacity of the antenna, which is a function of this frequency.

Q. The insertion of the condenser C of the capacity of .07 microfarads in the antenna circuit a of plaintiff's exhibit No. 68, would slightly reduce the capacity of the

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antenna, would it not? A. Very likely, although here the question is not very easy to decide. If you have no condenser in the antenna you have one node of potential, the earth, and in addition to that no other node is soon as you insert the condenser you have a node of potential on the part of the antennas between the condenser C and the upper part of the antenna. In such a case it is not very easy to say beforehand how the effective capacity of such an antenna is; it is not easy to decide what the effect would be.

Q. In answering did you bear in mind that the an-
10931 tenna capacity is only .001, while the capacity of the condenser C is .07, or seventy times as great? A. I did.

Q. If the inserted condenser has a very great capacity as compared to the effective capacity of the wire, its introduction has no appreciable effect upon the characteristic of the oscillations, has it? A. The higher the capacity of this condenser is the smaller, certainly, is the influence of this condenser. If you make this condenser C enormously large, then, of course, it would not have a material influence on the frequency or the current distribution in the antenna.

Q. My question, Dr. Zenneck, stated that the inserted condenser had very great capacity as compared with the
10932 effective capacity of the wire. What do you understand by "very great capacity?" A. That is very hard to say—to give a limit for that. But I would, certainly, consider a capacity which is seventy times as great as the capacity of the antenna as a very great capacity in this connection.

Q. Then, if I understand you correctly, the insertion of a condenser of so great a capacity would not appreciably affect the capacity or the time period of the antenna circuit? A. Yes, I do not think it would materially affect the frequency; although, of course, it will have an effect, and it would be very hard to say beforehand exactly how great the effect is.

Q. You stated that a capacity represented in the condenser c, seventy times as great as the effective capacity of the antenna, was great. Now, I understand from your small book, defendant's exhibit 62, pages 41 to 46, inclusive, that when this inserted capacity is very great there will be no appreciable alteration of the characteristics of the oscillations and, hence, there would be no node produced other than that at the ground; am I right? A. No.

Q. Why not? A. I do not know from which part of 10934
my book you draw this conclusion.

Q. Pages 41 to 46. A. Will you please show me the place? Will you be kind enough to tell me from which part of these five pages you draw the conclusion that I say there that in such a system there will be no node of potential except the earth?

The Court: Cannot this be looked up? I think we are not making the speed this morning we should.

Mr. Betts: If the court please, the matter is very highly technical and naturally I need assistance. 10935

The Court: I wondered if this could not be arranged while we proceed with something else. I do not want to interrupt the examination—it was simply a thought of mine.

Mr. Betts: I will pass then to another subject, in order to save time.

The Court: Very well, if it does not interfere with you.

Mr. Betts: About what is the maximum voltage between the top of the antenna on plaintiff's exhibit No. 68, and the ground, or the voltage

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across the dummy condenser of Dr. Chaffee's dummy antenna?

A. The voltage?

Q. Approximately, the maximum? A. I don't know.

Q. Well, it would be forty or fifty thousand volts or something like that? A. That is just a guess, I don't know.

Q. Well, can you give the court any idea—thirty or forty or fifty thousand? A. I cannot say for you any number. It is just a guess. I do not know. I have not

10937 measured it.

Q. The potential? A. The potential, yes—you know that the voltage is decreasing just as well as the current does, and I do not know how to answer this question. I do not even know the facts on which I can base the question, nor do I know exactly what you mean by it.

Q. Well, it would be very high, would it not? A. Why, I would not like to touch it.

Q. Then you would not be able to say what the charging potential of the condenser C of the Simpson mercury valve transmitter was? A. Will you please repeat that question.

Q. I say, you would not be able to say what the charg-

10938 ing potential of the condenser C was? A. No—you mean C^1 or C?

Q. I mean C. A. I think that is somewhere stated in Dr. Chaffee's testimony, what the potential, approximately, was. I do not know it exactly, but it must be in the record. There was some statement about this potential.

Q. Referring to the Simpson mercury valve transmitter as illustrated in plaintiff's exhibit No. 68, will oscillations in the circuit 2A, G, C, DD cause oscillations in the antenna circuit F, A, G, DD, C and E? A. I think that you cannot get oscillations in the circuit C, G without having at the same time in the antenna circuit A, C, the ground.

Q. But is it not true, Dr. Zenneck, that oscillations in the first named circuit will cause oscillations in the antenna circuit? A. Mr. Betts, you cannot make oscillations in this circuit, C, G, without having oscillations at the same time in the antenna circuit A, C and the opposite, you cannot make oscillations in the antenna circuit A, C without having at the same time oscillations in the first circuit, supposing, of course, that the gap is in oper-
ation and conductive. 10940

Q. Then, according to the definition in your small book, defendant's exhibit No. 62, page 79, these two circuits are coupled together, are they not? A. If you understand that as meaning that they are influencing each other, that the oscillations in one are dependent on the oscillations in the other, you are correct. If you understand this term as meaning that the system here is identical with the magnetically coupled system, to which the statements in my book refer, then it is wrong.

Q. Well, the two circuits in the Simpson mercury valve transmitter illustrated in plaintiff's exhibit No. 68, are directly coupled together through the coil dd', as de-
scribed by you on page 80 of your small book, defendant's 10941
exhibit 62, is that right? A. Yes——

Q. And then—— A. I did not answer that question.

Q. I thought you had, I beg your pardon. A. No, excuse me. On page 80 in my book I represented two circuits which are conductively, or directly coupled together, and for which the initial conditions, as I explained with reference to the chart Z-5, are the same as for magnetically coupled systems. Therefore, the circuit connection on page 80 of my book is equivalent to or practically identical with two magnetically coupled systems. The circuit connection on plaintiff's exhibit No. 68 is different from that in two respects: first, there is not only a conductive coupling here— —

Mr. Farnsworth: At dd[1]?

A. (Continuing) By the part dd[1], but also, so to say, a capacity coupling, as this condenser here is common to both circuits.

Mr. Farnsworth: Condenser C?

A. (Continuing) By the condenser coupling C, this
10943 condenser is contained in both circuits. Second, the initial conditions are different from those in magnetically coupled systems. I may add, that the capacity coupling and the magnetic or conductive coupling have not an additive effect; they have, so to say, a subtractive effect, as the capacity coupling in many respects is opposed to the direct coupling or magnetic coupling. I think I have explained that very fully in exhibit Z-5.

Q. Now, Dr. Zenneck, answer this question; is it not a fact that the two circuits in the Simpson mercury valve transmitter, as illustrated in plaintiff's exhibit No. 68, are coupled together by the coil dd[1] shown in that figure?
10944 A. If you had the condenser C, for instance, in the lead S1, S2, the system would, of course, be a system with conductive coupling by dd[1], and the conditions would be exactly those of magnetically coupled circuits. I just explained that, but here the conditions are different from this ordinary conductive coupling, in two ways; first, by the insertion of the condenser C, and second; by the difference in the initial conditions. The expression "coupled circuits" is so general, so indeterminate in such a connection, that I can say, with the explanation given, "no". I think I have explained everything in the direct examina-

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tion. In my deposition I tried to make it as clear as possible.

Q. But is it not a fact, Dr. Zenneck, that the portion of the coil opposite dd^1, in plaintiff's exhibit No. 68, does couple the antenna circuit to the spark gap circuit? A. This coil—you can just as well ask, of course, whether the condenser C does couple—

Q. I will come to that later; will you please answer my first question? A. There is no question that a current flowing through the coil dd^1 produces an effect both in the antenna circuit and in the circuit C, G, or that there is
mutual inductances between those two circuits; and if 10946
you want to call that "coupling", it is all right, but it does not mean more than that.

Q. In your small book, defendant's exhibit No. 62, at page 79, you have written a chapter here entitled "Coupled Circuits"—— A. Yes.

Q. (Continuing)——"When coupled in general. 52. Magnetic, galvanic, electric coupling. Two electromagnetic systems (oscillators or closed current circuits) are said to be 'coupled' if they are so arranged that oscillations in one of this circuit always cause oscillations in the other. That system or circuit in which the energy
is first supplied, say from the induction coil or some like 10947
source, is called 'The primary circuit', the other being called 'The secondary circuit' ". Do you so state in your book? A. Yes.

Q. Now, then, referring again to plaintiff's exhibit No. 68, the antenna circuit A, C to ground or earth, is coupled to the spark gap 2A, G, C, D, D^1 by means of the condenser C, is that correct? A. I again state that this is correct, if you understand that the word "coupled" means nothing else than that the two currents or circuits are influencing each other.

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Q. It means that the circuits are coupled together, just as you used the phrase in page 79 of your book, as I have just quoted it to you? A. Yes; it means with the same general meaning.

I may further state that in my book, as far as I know, in no place have I considered circuits like this here, but in the general sense in which I use the words "coupled circuits" on page 79, I would certainly have called that coupled circuits in the general sense But I would have never stated that those circuits here are identical with magnetically coupled circuits.

10949 Q. You have, I believe, stated a moment ago that the two couplings between the antenna circuit and the spark gap circuit on plaintiff's exhibit No. 68, are subtractive and not additive? A. In the same way. That is, perhaps, the most general expression of the effect of magnetic coupling and capacitive coupling.

Q. Now, the Telefunken transmitter which you referred to, that used a direct coupling between the quenched spark gap circuit and the antenna circuit? A. Some of them.

Q. And in these telefunken transmitters using direct coupling, did the quenched spark circuit transfer the en-
10950 ergy to the antenna circuit? A. If the direct coupling
is made in the way I have shown in Z-5, Fig. 1, of course the condenser C^1 replaced by the antenna capacity, then this connection is practically identical with magnetic coupling, and there is certainly a transfer of energy from the primary circuit C, G, PQ to the secondary circuit C^1, RP to P. I called it a transfer of energy, because the energy at the beginning, when the condenser C is charged is located in the electric field here inside this condenser. When this primary circuit is quenched there is only energy in the secondary circuit, and if we consider a moment

where the current in this circuit is zero, the whole energy is now in condenser C^1. I think that "transferring" means moving a thing from one place to the other, and so in this case——

Mr. Farnsworth: Fig. 1 of Z-5?

A. Fig. 1 of Z-5. In this case the energy has been moved from the condenser C to the condenser C^1; it was first in the condenser C, afterwards in the condenser C^1. That is the situation here. I call this a transfer of energy.

Q. (Mr. Betts) Before the oscillation started, was there any energy in the antenna represented by C^1 in 10952
Z-5? A. Before the oscillations started, there was no energy in the condenser C^1, as this condenser for static charges is short-circuited, so to say, by the lead QP.

Q. Now, then, suppose you connect the leads shown in the dotted lines on Z-5 across the spark gap, what happens then? A. Across the spark gap?

Q. Yes. A. Nothing else; just the same. They are connected across the spark gap. The connection here at the point which I call now a, is conductively connected through the lead QP, b with the point a^1. It is, therefore, absolutely immaterial whether the lead a is at the place a or at the place a^1; in the second case the leads 10953
going to the feeding current would be across the spark gap.

Q. Does the condenser C^1, representing the antenna in your drawing Z-5, get any energy before there are any oscillations? A. Certainly not.

Q. Now, referring to your small book, defendant's exhibit No. 62, will you please explain Fig. 45 on page 44. As I understand it, there is a node at the earth and also above the condenser, is that right? A. Yes, that is right.

Q. And you speak of that as an alteration of the os-

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cillation, as compared to figure 44, do you not? A. As an alteration?

Mr. Farnsworth: Do you understand that?

A. I don't know which one you refer to.

Mr. Betts: I am comparing Fig. 45 with Fig. 44.

Mr. Farnsworth: Your question is——

(Question read by the stenographer)

10955 A. I have shown in Fig. 45 that the distribution of current and potential is different from that in Fig. 44.

Q. Is it or it is not, Dr. Zenneck, an alteration of the oscillation, as compared with Fig. 44 of your book?

Mr. Farnsworth: You understand what he means by "alteration"?

The Witness: You mean "difference".

Mr. Farnsworth: Explain what you mean by alteration.

The Witness: I do not know exactly what you mean by the term "alteration". I say it is different, and I mean that the distribution of current is different. I beg your pardon, Mr. Betts, I do
10956 not know the exact meaning of "alteration"—is that identical with "difference"?

Mr. Betts: Different.

A. They are different.

Q. And you say on page 44 of your book, that if the inserted condenser is large it has no appreciable effect; is that correct? A. What page?

Q. Page 44, paragraph C. A. I say there:

"If the inserted condenser or condensers have very great capacity as compared to the effective capacity of the wire their introduction has no appreciable effect upon the characteristics of the oscillation, independently on the point at which the condensers are added''.

This means that the greater the condenser is, the less is the effect it has on the oscillations. I have stated here and shown in the figure, that a condenser has an influence on the oscillations. What I have wanted to state by the sentence just read is that the influence is the smaller the greater the capacity of the condenser is. I want to say, on Z-17, and I will show a condenser circuit—— 10958

Mr. Betts: If the court please, our time is precious and I do not want Dr. Zenneck to make any sketch; I asked him whether he had not stated something in his book.

The Witness: Will you read my answer?

(Stenographer reads witness' previous answer).

A. (Continuing) I may add that any condenser in any circuit, when the capacity is infinite, has no effect. It works like a short-circuit, but when the capacity is not infinite it has an effect. The effect may be small if the 10959 condenser is very large, and it may be very great when the condenser is not very large. Generally, therefore, if you insert a condenser into an antenna it has an effect. It depends just on the capacity of the condenser as to how great the effect is.

Q. But you stated, on page 44 of your book, that if a condenser be inserted in an antenna having very great capacity it has no appreciable effect upon the characteristics of the oscillations? A. Mr. Betts——

Q. Now, did you make that statement, Dr. Zenneck, in your book? A. Of course I did. The——

That is all I want to ask in regard to that. A. Please I want to explain it.

Q. All right. A. ——that I used these indeterminate expressions "very great capacity" and "appreciable effect" that shows that I just wanted to state that the greater you make the capacity the less the effect is—I did not want to make a quantitative statement here.

Q. You also state on page 45 of your book, defendant's exhibit No. 62, that where the coils have the greater effect, the distribution of current and potential will be shown in Fig. 47? A. Yes.

10961 Q. "That is, there will be no node except at the earth"? A. That is correct.

Q. And you have already said that a condenser of .07 microfarads, in an antenna of .007 microfarads is very large capacity and has no appreciable effect, have you not? A. I have done that in the sense I just explained, but I want to state now definitely, as shown by my Figs. 45 and 48, both showing antennas having a condenser inserted in it——

Mr. Farnsworth: What are the pages of your small book.

10962 A. (Continuing) 44 and 45. They both show a node of potential in the part between the condenser and the upper part of the antenna and, therefore, this node always exists, but this node may go down so much if the condenser capacity is very great, that there is no more any material difference from grounding it at this point —the question is the same as with a counterpoise.

Q. (Mr. Betts) Have you said all that you want to say, and if so, we will pass? A. Yes.

Q. You have, in your direct examination, in referring, I believe, to the Simpson mercury valve transmitter, said that the oscillations persist for relatively a long time;

do you remember making that statement? A. You mean the oscillations in the so-called antenna circuit?

Q. Yes. A. I referred, of course, to the arrangement of Dr. Chaffee.

Q. Now, when you said, "relatively long time," you mean relative to what? A. Relative to—for instance, to the so-called primary, or using the expression "relatively long time" in the same way as I did in the direct examination in answer to a question of Mr. Farnsworth's, where I discussed the question of persistent and non-persistent oscillators. An oscilator having about 100 oscillations would, I think, always be considered in 10964
any part of physics as a fairly persistent oscillator.

Q. You also said that there were in the antenna a relatively large number of oscillations? A. Yes.

Q. Relative to what? A. That is the same—I mean just what I said.

Mr. Farnsworth: The same answer?

The Witness: The same answer.

Mr. Betts: Can you fix any line of division of demarcation between what you have called persistent oscillators and non-persistent oscillators?

A. I have already explained in the direct examination 10965
that this is impossible.

Q. Now have you in your direct examination, when referring to the Simpson mercury valve transmitter as having a persistently oscillating circuit, made that statement when the antenna was coupled to the spark circuit or associated with the spark circuit, or when the antenna was taken alone? A. I have shown by my figure Z-7, representing the oscillations when the antenna was associated with the Simpson mercury valve transmitter, and by my figure Z-9, representing the free oscillations of the antenna circuit, that there is no material difference

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between those oscillations. It is, threfore, immaterial whether you refer my statement to the antenna oscillations when the antenna was connected with the Simpson transmitter or whether you refer it to the case where the antenna is disconnected.

Q. Were you present at the University of Washington in July 22nd and July 24th while some tests were being made by Mr. Thompson on the defendant's so-called defendant's receiver? A. I was not.

Q. What do you mean by an periodic circuit? A. I explained that on my chart Z-6, and I have nothing to
10967 add to this explanation.

Q. What do you mean by tight coupling between two circuits? A. There is, of course, the same question as with the persistent or non-persistent oscillator. There is no limit between loose coupling and tight coupling.

Q. You mean there is no exact line of demarcation or division between when you can say two circuits are loosely coupled and when you can say two circuits are tightly coupled, that is what you mean? A. That is what I mean, yes. There are, of course, coupling degrees, which in connection with electro-magnetic oscillations would always be considered as loose, say a coupling degree of one half per cent, or one per cent. And there are
10968 coupling degrees which in connection with electro-magnetic oscillations would always be considered as tight, for instance, a coupling degree of thirty per cent. But it is impossible to give a line of demarcation between tight and loose coupling.

Q. Would a coupling of say seven or ten per cent between two circuits be, in your opinion, a loose coupling? A. Seven or ten per cent?

Q. Yes. A. This degree of seven or eight per cent is just such that it is intermediate between loose and tight coupling. It would depend, therefore, absolutely on the conditions whether you would call it loose or tight. It is intermediate.

Q. When you were at the Cruft High Tension Laboratory at Cambridge, Massachusetts, did you observe any evidence of arcing in the spark gaps when they were opened on the second day, July 4? A. You mean the afternoon of July 4th, is that right?

Q. Yes. A. I did not see any indication of it. On the contrary, they looked to me very fresh, or relatively fresh.

Q. How long would you let quenched spark gaps work
before you took measurements on a quenched spark trans- 10970
mitter? A. It is, of course, very hard to say just how long is necessary. That depends on the spark gap used, but at least many hours.

Q. Did you observe the gaps on the Simpson mercury valve transmitter when they were opened by Dr. Chaffee on July 4th, for the first time? A. I did, some of them, at least.

Q. Did you consider those spark gaps when they were then opened in proper condition for further tests? A. When they were opened on July 4th, in the morning?

Q. Yes. A. No, I did not, certainly not.

Q. Did you at that time tell Dr. Chaffee that you did
not consider the spark gaps in proper condition for 10971
further tests? A. I do not think I told anything to Dr. Chaffee, but it may be that I talked with him. I do not recollect that. I certainly considered them as not proper for further tests.

Q. In your direct examination you spoke of the fact that during Dr. Chaffee's tests there were brush discharges of the Leyden jar; did you call Dr. Chaffee's attention to the fact that you were of the opinion that this might possibly affect the number of oscillations in the primary? A I did not call the attention of Dr. Chaf-

10972

fee to anything in his experiments. I considered the situation was that Dr. Chaffee was the man who made the experiments, and I did not want to interfere in any way with his experiments.

Q. Then you did not suggest to Dr. Chaffee that he measure the frequency of the dummy antenna? A. No, I did not. As far as I recollect I did not suggest anything to Dr. Chaffee.

Q. You are a physicist, are you not, Dr. Zenneck, rather than a designing and constructing engineer? A. I certainly am.

10973 Q. How many transmitters actually designed by you have gone into regular commercial manufacture and use? A. I understand this question as meaning how many of the transmitters in actual use have been really designed by me, is that correct?

Q. Yes. A. No.

Q. You mean none? A. None—I beg pardon.

Q. When did you come to this country? A. December, 1914, the end of December.

Q. For what purpose did you come to this country? A. To help the patent attorney of the Atlantic Communication Company case, Mr. Knight, in preparing the evidence in a suit brought by the Marconi Company
10974 against the Atlantic Communication Company, and in order to testify in this suit.

Q. And that suit is the suit we have heretofore referred to as having been tried before Judge Veeder on the two patents here in suit? A. Yes.

Q. Marconi patents? A. Yes.

Mr. Farnsworth: Partially tried.

Q. Well, it is the case that you testified in? A. Yes.

Q. Who asked you to come from Germany to America at this time for this purpose? A. The patent attorney, Mr. Knight, has asked the Telefunken Company to ask Professor Braun and me to come over to this country in order to testify in the suit mentioned.

Q. And the Telefunken Company that you refer to is a German company known as the—— A. Gesellschaft fur drahtlose Telegraphic.

Q. And the Atlantic Communication Company uses the Telefunken apparatus, or apparatus made by the Telefunken Company that you have mentioned? A. I do not know whether all apparatus used by the Atlantic Communication Company are manufactured by the Ge- 10976
sellschaft fur drahtlose Telegraphic, but I know that at least a great number, perhaps the greatest number of them are.

Q. And the Atlantic Communication Company operates the Sayville station in conjunction with a wireless station at Nauen, in Germany, operated by the German Telefunken Company? A. Certainly.

Q. Who paid your expenses to Boston when you witnessed Dr. Chaffee's tests at the Cruft High Tension Laboratory? A. The Kilbourne & Clark Company—you mean the living expenses and trip?

Q. Yes. A. Yes.

Q. And the Kilbourne & Clark Company are paying 10977
your expenses while out here? A. I expect so.

Q. Or is it the Atlantic Communication Company? A. I expect the Kilbourne & Clark Company to do it.

Q. And is the Atlantic Communication Company paying you a salary while you are in the United States? A. No, only my living expenses.

Q. And before you came out here to Seattle to testify here you obtained permission or authority from Mr. Knight, counsel of the Atlantic Communication Company? A. Yes, he agreed to that.

10978 Dr. J. Zenneck—Recalled—Redirect.

Q. Have you any objection to stating, Dr. Zenneck, whether or not you are an officer in the German Army at the present time? A. I have no objection whatever. I had to apply for being dismissed from the German Navy before I left, and I am no more a reserve officer of the German Navy at the present time.

Q. As I understand your last answer, you obtained leave, did you not, to come over? A. I cannot obtain leave during the time of war. I had to apply for being dismissed from the Navy.

Q. In order to come over to New York? A. In order
10979 to come over here.

Q. And in your testimony before Judge Veeder in the Atlantic Communication Company case you contended that the Telefunken apparatus was of the impulse transmitting type, did you not? A. I certainly did.

Mr. Betts: That is all. Cross examination closed.

Redirect Examination.

Q. (Mr. Farnsworth) In a recent question of Mr. Betts' he asked you whether or not the day before yesterday, this week, July 24, you attended the tests of re-
10980 ceivers of the defendant at the University of Washington. Did you so understand the question? Were you or were you not at the University of Washington this week Monday, at the defendant's receiver tests? A. This week Monday?

Q. Or Saturday, the 22nd? A. I understood the question as meaning whether I took part in this experiment. I was present one day at least. I was present on the morning down there in the laboratory of the Kilbourne & Clark Company, and I was present the same afternoon from about 4 o'clock at the University of Wash-

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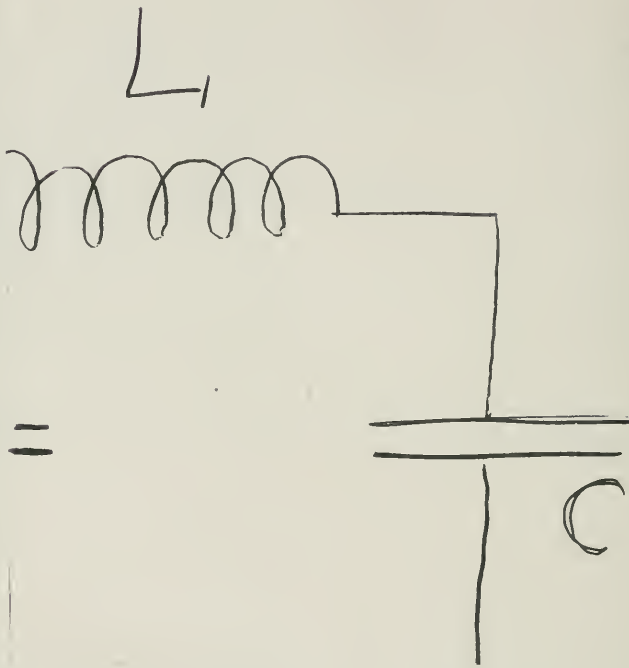

Fig

 10987

ington, but I did not take part in these experiments. That is how I understood the question.

Q. You were present and observed the tests at the times you stated? A. I watched them, that is, I just saw that they were measuring something, nothing else.

Q. I will now give you an opportunity briefly, if you please, to finish what you commenced on cross examination in drawing Z-17 (reproduced opposite), and briefly state what you wished to state in that connection, concerning the effect of condensers on oscillations.

A. In order to explain the action of a condenser inserted into a circuit I wanted to explain that in an 10988
especially simple case, a condenser circuit consisting of a coil L, and of the condenser C. If into such a condenser circuit a second condenser is inserted, which I mark now by dotted lines and by C1, then this condenser C1 has generally an effect on the frequency. Strictly spoken it has always an effect on the frequency of this condenser circuit. But if the capacity of this condenser is very great, then the effect is very small. Therefore, in this very simple case, which is very easy to calculate exactly, the situation is the following: If you insert a condenser C1 the frequency always changes; but the change of frequency is the smaller the greater the capacity of this condenser is. If the capacity of this condenser is 10989
very great compared with the capacity of the condenser C, the result may be expressed in the words; the condenser C1 has then no more any appreciable effect on the frequency of this condenser circuit. That is all I wanted to express.

Q. You have stated that in a condenser circuit, containing a spark gap the decrement does not depend on the capacity or inductance of the circuit, but only on the properties of the spark gap. Does your statement hold good for all condenser circuits containing spark gaps?

Z 17

July 26 16

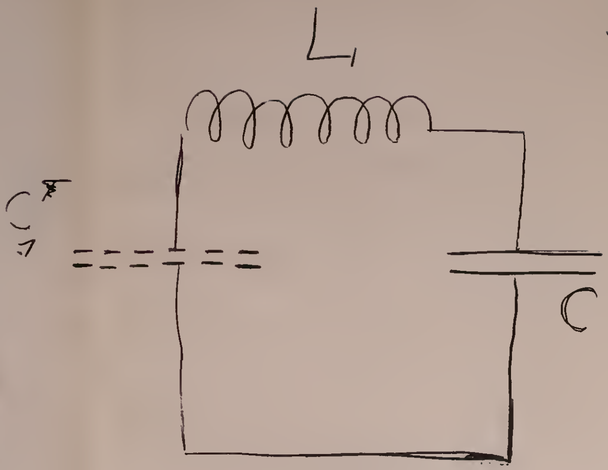

Fig 1

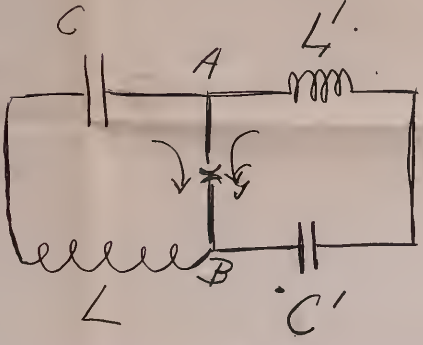

Fig 2

A. I have already explained that in connection with a question which I was asked by Mr. Betts on the cross examination. It refers only to such circuits with reference to which I have used it, namely in which the energy consumption in the spark gap is by far higher than the energy consumption at any other part of the circuit.

Q. If by measuring the current in one part of a circuit you find that there are beats, does that prove that the beats are produced by the interaction of two circuits? A. If you measured the beats only in one part of the circuit, the existence of these beats does not necessarily
10991 prove anything about an interaction of two circuits. I may show a case on the same chart, Z-17, Figure 2. (Witness draws Figure 2.) I have represented two condenser circuits, the one containing capacity C, and the inductance L, and the other containing the capacity C^1 and the inductance L^1. The common part, AB, of the two circuits may be assumed as containing no appreciable inductance, just the spark gap G. Then there is no appreciable reaction between these two circuits. If then the frequency of the circuit C L is different from the frequency of the circuit C' L' we would get in the lead AB two currents, one from the one circuit, and the other from the other, therefore, two currents of different fre-
10992 quency. These two currents of different frequencies would give beats, as currents of two frequencies always give. In this case, therefore these beats would not give any indication of an interaction between these two circuits. They would be produced by the difference of the free oscillations of these two circuits.

I may add that such an arrangement is, for instance, contained in the American patent 1087126, by B. Macku, L. Mandelstam and N. Papolexi.

Q. In answer to a cross question you stated that the properties of the spark gap constituted one of the fea-

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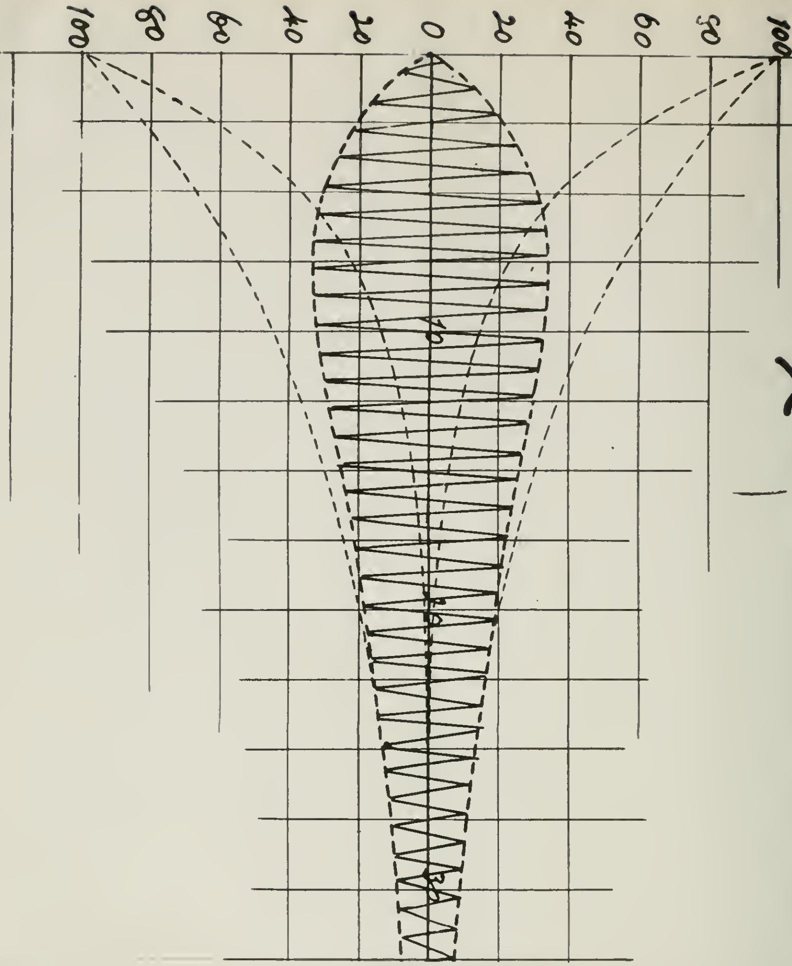
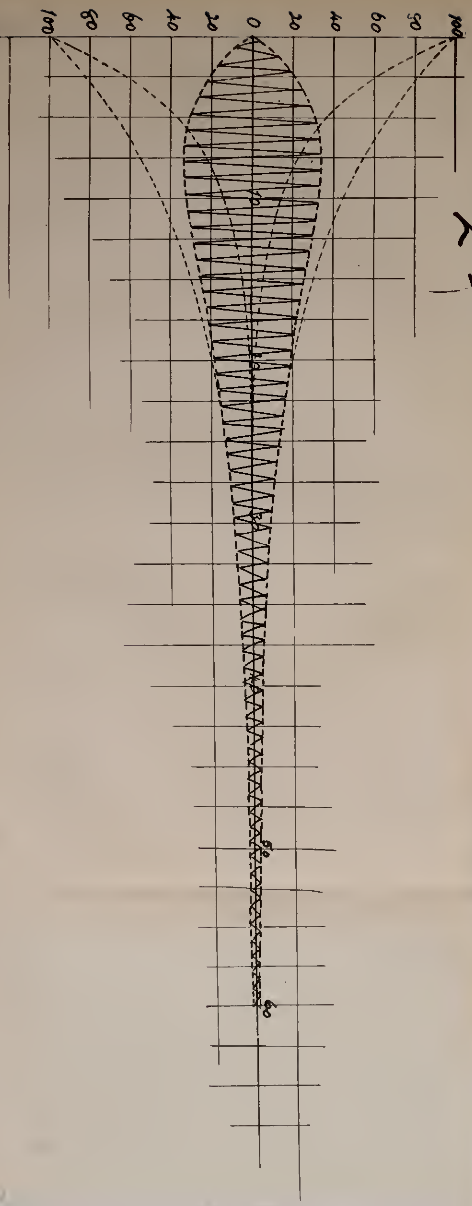


 10999

tures of difference between the two types of transmitters, namely, the impulse type and the coupled circuit type. Please state what are the other features of difference, if any. You may simplify your answer by reference to any diagrams you have had, including Z-16. A. As to the physical properties of these two systems I see the main difference in the following points:

1. The spark gap which I already mentioned. 2. The fact that the coupling plays an entirely different part in the impulse than in the coupled circuit transmitter. In the impulse transmitter the coupling degree is very critical as it affects the properties of the beats, and by this 11000
way the quenching action of the gap. It may, therefore, be that a small change of the coupling degree changes the energy of the radiated waves by a very great percentage. No such relation exists in the coupled transmitter. 3. The tuning plays a different part in both transmitters. As far as I know, in the coupled circuit transmitter the tuning is very critical, that is to say, a small detuning generally means a relatively great decrease in the energy of the waves emitted. In impulse transmitters, at least in those I know, the tuning is not very critical, and a detuning of some percents may even give a better result than an exact tuning. 4. As to the result,
an impulse transmitter radiates waves of the kind I have 11001
shown, for instance, in chart Z-7 and chart Z-8. These waves or antenna oscillations are practically identical with the free oscillations of the antenna. It is, therefore, justified to characterize the impulse transmitter by the statement that here practically the free oscillations of the antenna are radiated. In the coupled circuit transmitter the situation is this. Using a very loose coupling the increase and decrease of the amplitude of the antenna oscillations is of the kind that I have represented on chart which I mark Z-18 (reproduced opposite).

3666



11002 Dr. J. Zenneck—Recalled—Redirect.

The Witness: This decrease or increase of the amplitude is defined in this case by the constants of the primary as well as of the secondary circuit. If a very tight coupling is used, two oscillations are produced, and, as far as I know, for this reason the use of this tightly coupled transmitter is no more allowed by International law, as producing two oscillations. By using intermediate couplings the form of the oscillations is intermediate between that represented on Z-18 and the oscillations with beats, an example of which I
11003 have diagrammatically represented on page 94, Fig. 130 of my small book. Therefore, this transmitter never emits the free oscillations of the antenna.

Q. One more question, Dr. Zenneck, please; on your cross examination you calculated, at Mr. Betts' request, about ten oscillations by the Bjerknes formula for the converting trigger circuit of the Simpson mercury valve transmitter. Will you please state what bearing that result has on the question of the difference between the two types of transmitters, namely, the impulse type and the coupled circuit type? A. I think it has no bearing what-
11004 soever, because I have explained in—I think the cross examination that this method of calculating the number of oscillations is incorrect, in view of the assumptions made.

The Court: I think your reference is now to your past testimony.

A. Yes, I think it is all covered by my testimony.

Mr. Farnsworth: That is all, Dr. Zenneck. The defendant offers the charts, Z-15, Z-17 and Z-18 as added parts of defendant's exhibit No. 64, which included the Zenneck charts and blue prints Z-1 to Z-14. That is all of the redirect.

(Zenneck charts Z-15, Z-17 and Z-18 added to and made a part of defendant's exhibit No. 64.)

RECROSS EXAMINATION.

Q. (Mr. Betts) Dr. Zenneck, you have assumed in your redirect examination and in your whole testimony here that a quenched spark gap is not used in a coupled circuit transmitter. A. I certainly did that. 11006

Q. Now, what do you mean by saying in your direct examination that the tuning is not very critical. That is the phrase you used, did you not? A. Yes, I did.

Q. What do you mean by that, "not very critical"? A. I mean by this the following; I meant that for a certain percentage of detuning the energy of the secondary circuit is relatively not much smaller than that for exact tuning. May I express it in this way; that the so-called resonance curve for these two circuits is not sharp, but more flat. That means, of course, the same.

Mr. Betts: That is all.

11007

Q. (Mr. Farnsworth) Did you state all you wished to to Mr. Betts' first question as to the quenched spark gap not being used in coupled circuit transmitters; did you say all you wished to as to that? A. I have assumed in my testimony, and have expressed it as far as I recollect, that I considered a coupled circuit transmitter as one containing a spark gap which has no material quenching effect. I think I did that in my cross examination.

Mr. Betts: I have nothing further.

Mr. Farnsworth: Have you finished that?

A. I have finished that.

(Witness excused.)

Frederick A. Kolster, recalled as a witness on behalf of Defendant in Surrebuttal, testified as follows:

Q. (Mr. Farnsworth) Mr. Kolster, you have been a witness before in this case? A. I have.

Q. You were present at the plaintiff's Massachusetts
11009 tests on July 3 and 4 last? A. I was.

Q. Will you briefly state your conclusions as to your observations of those Massachusetts tests? A. It would be really using up valuable time unnecessarily for me to go into the details of the tests in my own words, as Professor Zenneck has covered the ground very carefully, as well as Mr. Stone and Mr. Simon, who also were present at the tests, have done in their testimony.

Q. And Mr. Pickard, you have read the testimony of Mr. Pickard? A. And Mr. Pickard.

Q. You mean John Stone Stone? A. John Stone Stone. There were one or two points I would like to men-
11010 tion, one of the most important of which is the fact that while I was present at these tests a very important measurement was not made, and that is the measurement of the decrement, and the obtaining of resonance curves in the antenna circuit. I think those measurements would have given us a great deal of information which would have made the result of the tests very much clearer.

Q. Did you state whether or not you agreed with the conclusions expressed by Messrs. Simon, Stone, Pickard and Zenneck as to the plaintiff's Massachusetts tests? A. Yes, I agree with those conclusions absolutely.

 11011

Q. And you have completed your statement of your own? A. Yes.

Q. What did those Massachusetts tests show, if they showed anything? A. I think those tests showed, at least the results of those tests corroborated to a great extent the results of the Bureau of Standards tests, so far as they went, and to my mind clearly proved the fact that the Simpson mercury valve transmitter as operated by Dr. Chaffee was an impulse transmitter.

Q. Were you present and did you observe the defend ant's receiver tests on July 22 and 24 last? A. I did.

Q. State briefly what those tests showed? A. The re- 11012
ceiver tests which were conducted on the days you mention showed that the efficiency of the receiving circuit of Kilbourne & Clark with so-called untuned detector circuit was as good, and in many cases much better than the same receiver with the condenser connected in this detector circuit in order to make it a tuned circuit. The tests also showed, as I stated in my previous testimony, that no advantage whatever was derived from any resonance effects existing in this detector circuit. In fact, in some tests a new coil was substituted for the regular standard detector coil, which was wound to have a natural period corresponding to an extremely short wave- 11013
length, and this coil showed even better results than the standard coil, which was wound to have a natural period corresponding to a longer wave-length. And in conclusion, the tests as a whole showed that the operation of the receiver is entirely independent of any resonance effects in the detector circuit, but its operation simply depends upon the coupling between the resonance antenna circuit and the untuned detector circuit.

Mr. Farnsworth: You may cross examine.

11014 Frederick A. Kolster—Recalled—Cross.
F. G. Simpson—Recalled—Direct.

CROSS EXAMINATION.

Q. (Mr. Betts) Mr. Kolster, are you while out here at Seattle still on duty as a member of the force of the Government Bureau of Standards? A. I am.

Q. And you assisted Mr. Thompson during the tests of July 22 and July 24 on the defendant's receiving apparatus? A. I occasionally listened in and made adjustments.

Q. And you were at the Washington University on
11015 Sunday, the 23rd, with Mr. Thompson? A. I was.

Q. How long were you there? A. I think about two hours.

Q. And the purpose of your visit being to set up and adjust the defendant's apparatus preparatory to the tests of July 24. A. I merely went there with Mr. Thompson while he did that.

Mr. Betts: That is all, if the court please.

(Witness excused)

11016

Afternoon Session 2 o'clock p. m. Continuation of proceedings pursuant to recess. All parties present as at former hearing.

F. G. Simpson, recalled in behalf of the defendant, in surrebuttal testified as follows:

Q. (Mr. Skeel) Mr. Simpson, you have already been sworn to testify in this case? A. Yes, I have.

Q. Did you attend the plaintiff's tests of the Simpson transmitter in Massachusetts? A. I attended the tests that were run in Massachusetts, in which part of the Simpson transmitter was used.

Q. Will you state very briefly, and without any unnecessary duplication, what you saw at said tests? A. On the morning of July 3rd I went out to Cruft's Laboratory at Harvard University and I saw that Professor Chaffee had set up a Simpson transmitter panel consisting of the marble panel with its instruments, wires and so forth, and the regular transformer that was used and with that panel and attached to the panel, that he had provided a 500 cycle alternating current generator.
That is, it was a motor generator, but the generator part 11018
of it was as I defined it; the voltage of which was one-half of the normal voltage used in connection with my transmitter. That is, the voltage, according to the testimony given by Professor Chaffee, was, approximately, 110 volts. I do not remember whether he said 110 or 120 volts. I have never used a 500 cycle generator to furnish energy for my transmitter at that voltage. The voltage that I have used being, approximately, double that. This generator bore the name plate of the Marconi Wireless Telegraph Company of America, and also upon that name plate there was a patent notation, showing that it
was a patented generator, presumably, because of some 11019
special feature in connection with it, of which I am not aware. However, the voltage was only one-half of the normal voltage used with my transmitter. The generator was connected to the transformer through a reactance—a reactance of a kind of which I have no knowledge. I use my transmitter myself with the normal sort of reactance; and this may or may not have been such —I do not know. The antenna used was a dummy antenna made up in the conventional way, with the exception that Leyden jars were provided as a condenser

within that antenna, and those Leyden jars did brush excessively throughout the entire series of tests.

The tests were run during the day of July 3rd and were not finished upon that day; so that we returned on the morning of July 4th. We were informed that it had been discovered that the spark gaps had been badly damaged sometime previous; whether that time was July 3rd during the tests, or whether it was during the many days in which the transmitter had been operated, or in the possession of the plaintiff in this case, I do not know.

11021 Mr. Betts: I think this is a little argumentative; he should stick to the facts.

The Court: Oh, yes, I think so.

Q. (Mr. Skeel) Just confine yourself strictly to what you saw, and do not duplicate the testimony of other witnesses, if you can avoid doing so. A. The gaps were shown to us. Two of them had been very badly burned and I was informed, had been welded together. In separating the gaps the insulator had been broken in many places. The gaps were in process of repair when I saw them. Some work had been done upon them. Those gaps were re-assembled and an attempt was made to
11022 make them air-tight by using sealing wax; the sealing wax was poured in between the insulator and the metal backing of the gap.

Tests were run during July 4th, and photographs were taken. I might say that upon July 3rd I listened in with the wave meter; that is, a wave meter connected as a receiver, to the note of the instrument, and at all times when I heard it the note was very bad. Upon July 3rd the note was somewhat better, but still very bad. I did not hear a clear note at any time. I did not hear a note at any time that either I or any of my inspectors would pass if the set were installed upon shipboard. The tests run during July 4th were of the same nature and order as those run on July 3rd; that is what I saw.

Q. Now, assuming that said tests correctly showed the action of the transmitter used in Massachusetts, what are your conclusions therefrom, in reference to the characteristics of your transmitter? A. Why, under the extremely abnormal conditions under which the apparatus was operated at the tests which we are discussing, the results shown are positive. They confirm my contention, that the transmitter is something radically different from any transmitter of which I have knowledge, and 11024
that the transmitter operates exactly as I have said it does in my previous testimony.

Mr. Skeel: You may cross examine.

CROSS EXAMINATION.

Q. (Mr. Betts) What do you mean by saying that you witnessed certain tests at the Cruft High Tension Laboratory, conducted by Dr. Chaffee, with a part of the Simpson transmitter? A. I mean that the alternating current generator, which forms a part of the Simpson Mercury Valve transmitter, was not used at all during the tests 11025
at Cruft's Laboratory, although such generator had been furnished to the plaintiff. I mean also that the tests, in so far as they were considered official by the plaintiff, on July 4, were used with spark gaps in an entirely different condition from any spark gaps of which I have ever had knowledge as having been used in connection with the Simpson mercury valve transmitter.

Q. Do I understand you to state now that a motor generator is a constituent part of the Simpson mercury valve transmitter set? A. Certainly, a motor generator is a constituent part of it. The motor generator which——

Q. That answers my question. A. Yes.

Mr. Skeel: If there is anything which you consider necessary to add to make the question clear——

Q. I would like to, yes.

Mr. Skeel: You may do so.

11027 A. (Continuing) It is true, that the source of power which I have been in the habit of using may be either a 500 cycle generator or a 120 cycle generator, or you might even use a 60 cycle generator, and that there may be many generators available aside from those which are manufactured by my company, which would be suitable, but, certainly, no generator producing a pressure of one-half the pressure, or voltage, that I used can be used in connection with tests of the nature of which these tests were presumed to be.

Mr. Betts: I think that is argument, if the court please; I asked him whether a generator was
11028 a constituent part of a Simpson transmitter.

The Court: He answered it.

Mr. Betts: That is all.

Mr. Skeel: That is all.

(Witness excused.)

Mr. Skeel: It is agreed that Mr. Waterman shall submit a comparative list of new devices in this case as compared with the National Electric Signaling Company case, and that that shall be a part of the record in this case.

 11029

The defendant now rests, if the court please, except that I am not clear whether Mr. Betts wishes to cross examine Mr. Thompson—Mr. Thompson, if you recall, having been on the stand; and Mr. Wolff also; I understood Mr. Hughes wishes to recall him.

Mr. Betts: Mr. Hughes will want to cross examine Mr. Wolff.

Mr. Skeel: With that exception, the defendant now rests.

The Court: Call the witnesses that you now want to cross examine. 11030

Mr. Betts: I would like to call Mr. Thompson.

R. E. Thompson, resumes the stand for further cross examination.

Q. (Mr. Betts) I believe, Mr. Thompson, that you used in your direct examination the phrase "impulse charging circuit"; what did you mean by that phrase? A. I meant a circuit which substantially transfers all of its effective energy to the antenna circuit in one oscillation.

Q. One complete oscillation? A. Either a half oscil- 11031
lation or part of another one.

Q. Well, which do you mean? A. Either.

Q. And would you say that an impulse charging circuit was one in which the energy was transferred to the antenna circuit in two oscillations? A. I should say that it could be so considered.

Q. Three oscilations? A. It would depend altogether on the antenna circuit with which it was connected.

Q. Four oscillations? A. What I meant by that is this: if two circuits are coupled together, and one transfers its energy to the other quickly, I would call the first one an impulse charging circuit. If, when those two circuits are separated and an electro-motive force is applied independently to each one and then suddenly cut off, the antenna circuit would persist longer than the closed impulse circuit.

Q. In other words, that the number of oscillations in the primary circuit is immaterial, so long as its number is less than the number of oscillations in the antenna circuit. A. If, when these two circuits are separated, as I said, and the energy applied in the manner I just spoke of in the preceding answer, and if then the two circuits
11033 are associated together, so long as the energy in the primary circuit is materially less, as regards the number of oscillations, than that in the antenna circuit, yes; such a circuit could not then be a reservoir circuit to the antenna circuit, under my definition.

Q. (Mr. Betts) Now, you have also used the expression in your direct examination "pure impulse excitation;" what did you mean by that phrase? A. Pure impulse excitation is defined right in those words, but I might go still further and give my idea of what it means, and that is this: any means of quickly imparting the energy to an antenna circuit and then cutting off the impulse circuit so that the antenna circuit is free from any
11034 effect due to the impulse circuit.

Q. Are you at all familiar with the theory of coupled circuits? A. Well, I do not know that I am thoroughly familiar with the theory; I am familiar with the action of coupled tuned circuits.

Mr. Betts: That is all.
Mr. Skeel: That is all.

(Witness excused.)

 11035

The Court: Call the next witness—I believe you have not completed the cross examination of Mr. Wolf, Mr. Hughes.

Mr. Hughes: I would like to call Mr. Wolf.

B. Wolf resumes the stand for further cross-examination.

Mr. Hughes: To save time, and for the purpose of explaining the occasion of the defendant's continuance of the cross-examination, I may state for 11036
the purpose of the record, that I have examined the records in Mr. Wolf's office in the radio room, that will show that it is equivalent to his complying to my request—I requested that he produce them. Having done that, and in order to avoid any unnecessary encumbering of the record, I do not claim any advantage by reason of producing, or asking him to produce them, admitting that I have seen several of the other records.

Q. Mr. Wolf, turning to your record designated, "Radio Apparatus Adjustment Record," introduced in 11037
evidence as defendant's exhibit No. 57, and describing the primary coil, you designate the diameter of the helix as nine inches; that refers to the diameter of the loop of inductance in the primary coil, does it? A. It does.

Q. Now, referring to the wave lengths, you give the number of units in the primary condenser as, how many? A. The number of units in the primary condenser is two, in series.

Q. You examined, that is, you took off the cover and examined the condenser, for the purpose of making the inspection at the time this adjustment certificate was used, or licensed? A. (By the witness) If I remember correctly, I made this inspection, or I went on board for the purpose of making the inspection during the time that the set was being installed, and in this particular case the condenser cover was off, and it could be observed. I did note, however, that there was no change of any sort made in the primary or spark gap circuit for the change of wave lengths that the set was adjusted for.

Q. You measured the antenna current for the 600 meter wave? A. I did.

Q. (Mr. Hughes) And also for the 300 meter wave?
11039 A. I did.

Q. The current, as shown by this chart, for the 600 meter wave, was five volts? A. Five amperes.

Q. Five amperes, I should say? A. That is right.

Q. And for the 300 meter, one ampere? A. That is right.

Q. (Mr. Hughes) Returning to the radio apparatus adjustment record for the S. S. Alameda on July 25, 1915, being exhibit 58; in describing the primary coil you give the diameter as eight inches; that means the diameter of the loop of inductance which has been referred to in the testimony? A. It does; the loop of inductance in the primary circuit.

11040 Q. The loop of inductance in the primary circuit? A. In the spark gap circuit.

Q. In describing the oscillating circuit, the primary circuit; you describe the condenser as consisting of two units? A. That is right.

Q. (Mr. Hughes) Did you make any measurement of the voltage of the 300 meter wave—of the current producing the 300 meter wave?

Mr. Skeel: I object to that as improper cross examination.

The Court: He may answer.

A. I did.

Q. (Mr. Hughes) What was that voltage? A. One-half an ampere at 300 meters.

Q. About one-tenth of the voltage of the 600 meter wave? A. About.

Q. I mean, of the current? A. About one-tenth.

Q. Would that be efficient for the purpose of transmitting wireless messages? A. That would be efficient for transmitting wireless messages.

Q. Under all ordinary circumstances, would you say for distances of one hundred miles? A. It would depend
to a great degree on what was intervening between the 11042
station that was desired to be communicated with. Over water I would say that it was, probably, sufficient.

Q. You referred to an inspection of the records; without taking the time to go over that, I observe that a number of these records made by you show no examination of the current for the 300 meter wave; is it true that you did not——

Mr. Skeel: (Interrupting) I object to that as being improper cross examination.

Mr. Hughes: Well, these have been introduced in evidence.

Mr. Skeel: I already stated my position. The 11043
issue was raised by the plaintiff as to whether or not we, surreptitiously, provided, or permitted ship owners or operators to use additional loops. Now we simply had Mr. Wolf testify as to the practice of the government as to licensing apparatus and to show that if we had any loops that were not licensed, our licenses would be forfeited. Now, it seems to me, Mr. Hughes is going back to the merits of the case; and if he does it is necessary for us to go back.

11044

Mr. Hughes: As a matter of fact, if the court please, what our evidence tended to show was the possibility of varying the wave lengths, and the introduction of different styles of loops was simply for the purpose of showing one means of varying the wave length; and they offered evidence only tending to show that they did not vary the particular amount of inductance in the size of this coil in practice, but in attempting to do that they have not confined it to that; they have introduced the record showing the entire examination, and a de-
11045 parture in any other respect would equally subject them to forfeiture. Now, it seems to me that we are trying to get at the truth here, rather than to be technical, we ought to be at least, and if by this examination, or in any other way, we can show that, instead of varying in that respect, as in the size of this inductance coil, they had varied the wave length by some other means equally efficient for that purpose, which has been withheld from the knowledge of the court, it will be proper to show it, and I submit that since they have asked this witness about these examinations, and intro-
11046 duced them in evidence, they make it a matter of cross examination.

The Court: I think the objection will be sustained; note an exception.

Mr. Hughes: That is all.

Mr. Skeel: That is all. The defendant rests.

(Witness excused.)

(STATEMENTS OF COUNSEL, PLAINTIFF'S MOTION FOR SURREBUTTAL, NOTICES, OFFERS AND RULINGS.) 11047

Mr. Betts: May I, if the court please, offer evidence in reply to the surrebuttal evidence of the defendant?

The Court: If there is any evidence here that needs to be rebutted, why, of course, you may.

Mr. Skeel: I have no objection to anything new that we have said being rebutted, but I give notice now that I will object to attempts to bring back the issues that were fought out last April.

Mr. Betts: The court will remember the testi- 11048
mony given by Mr. Simpson a few moments ago, and also the testimony read to your honor the other day of Mr. Stone and Mr. Pickard, in regard to the use during Dr. Chaffee's tests at the Cruft High Tension Laboratory at Harvard University, of a 500 cycle generator with the Marconi Company's name plate thereon, and in reply to that testimony I desire to read into evidence, as an admission by this defendant, of two notices entitled in this cause, served upon the plaintiff's solicitors by the defendant's solicitors respecting the
mercury valve transmitter which they loaned us 11049
and which was subsequently tested by Dr Chaffee.

Mr. Skeel: Are these in evidence in this case—already in evidence?

Mr. Betts: They are not in evidence for this purpose, and therefore I am offering them as an admission by the defendant.

Mr. Skeel: Then I may ask that all the notices that comprise that group be read at the same time.

Mr. Betts: You can offer any that you please; I am offering these two which were sent by you.

11050 Defendant's Letters *In Re* Generators of Simpson Transmitter.

Mr. Skeel: If the court please, I just wish to give notice at this time then that for the same purpose we will rely upon all the notices comprising the correspondence on the particular subject referred to, but we deem it unnecessary to take up the time this afternoon, when that time might more properly be taken in introducing other evidence in the case.

Mr. Betts: I desire now to state that I consider that it is proper, while the matter is fresh in the
11051 court's mind, that I should read this notice.

The Court: Proceed.

Mr. Betts (Reading):

"Seattle, April 3rd, 1916.

Defendant's Letters In Re Generators of Simpson Transmitter.

"To E. C. Hughes
and
L. F. H. Betts,
Attorneys for Plaintiff:

"We acknowledge receipt of your communi-
11052 cation, dated today, relative to your partial acceptance of our offers to provide with facilities for examining our two types of transmitter, which offers were made on the record Saturday afternoon, April 1st.

"As Mr. Farnsworth telephoned Mr. Hughes at eleven a. m. today immediately upon receipt of your said alleged acceptance to our offers, the 'Simpson' transmitter remains installed at the laboratory of the defendant on Columbia Street, where it is awaiting removal by you for your purpose. This is a complete 'Simpson' transmitting set, exactly like the transmitting sets manufactured and sold by the defendant prior to the filing of the bill of complaint herein, and installed upon

 11053

the ships of the Detroit & Cleveland Navigation Company, and this particular set was the one which was measured and testified about on Saturday last by Mr. Simpson, and by Mr. Kolster of the United States Bureau of Standards, and it is now in the same condition as at that time. We prefer not to disturb this condition ourselves, but that you remove the apparatus.

"Respecting the standard 'Impulse' or 'Thompson' transmitting set, you may send today also for one of these exactly like the 'Impulse' transmitting sets manufactured, sold and installed by the defendant on the S. S. 'Admiral Evans' 11054
prior to the filing of the bill herein. The set is the one which Mr. Thompson exhibited and testified about at the hearing on Saturday afternoon, April 1st, at the defendant's laboratory, and as was testified at the time, this set was not then completely installed on account of the delay involved by our acceeding to the Marconi Company's request to discontinue work on it, in view of some ship distress business on which they were engaged Saturday morning.

"As we have informed you previously, only one transmitter like that on the S. S. 'Alameda,' and which set you have already tested, was ever manufactured by defendant, and, of course, have none in stock. 11055

"As sources of power for either or both the 'Simpson' or 'Impulse' transmitter, we assume that on account of your more extensive stock of generators, than ours, you have all generators necessary for your tests of these two transmitters. However, we offer you any or all of the generators we have in stock or can obtain for you, including a 5 K.W., 500 cycle generator, or a 1 K.W. 60 cycle rotary converter; also the 5 K.W. 500 cycle motor generator, which was used to furnish the power for the 'Simpson' transmitter, when Mr. Kolster measured the latter at the court's session on Saturday afternoon, April 1st;

11056 Defendant's Letters *In Re* Generators of Simpson Transmitter.

also any other generators, which you desire and which we can obtain for you. This morning, April third, we do not happen to have in stock or readily available any 120 cycle generators, as all those which we possessed are now in service on ships, supplying power to our transmitting sets.

"In view of Mr. Betts' letter of today, regarding a 120 cycle generator, which letter was received since the receipt of your alleged acceptance of our various offers, we are exerting our best efforts to obtain such generator for you and will notify you as soon as we obtain one if we suc-
11057 ceed.

"However, we are informed that the Marconi Company has in its possession at Seattle, a Robbins & Meyers 120 cycle generator, and in connection with this, we direct attention to the fact that we have turned over our two types of transmitter to you without restriction as to use by you, save that they be returned in good condition.

"We regret that on Saturday afternoon, April 1st, when our offers to you were repeated, Mr. Betts omitted to specify that you wished a 120 cycle generator. If he had not delayed to do this until this morning we would have been enabled to commence on Saturday afternoon our search for
11058 a generator of this number of cycles. Yours truly,

"Kilbourne & Clark Manufacturing Company, by E. L. Skeel, its Attorney." "Service of the above is hereby acknowledged this third day of April, 1916 ,at 4:26 p. m. L. F. H. Betts, Attorney for Marconi Wireless Telegraph Company of America."

Defendant's Second Letter In Re Generator of Simpson Transmitter.

Also notice, dated Seattle April 4, 1916, and entitled in this case, as follows:

"To E. C. Hughes
and
L. F. H. Betts
Attorneys for Plaintiff:

"Replying to your last letter of yesterday, received this morning, we state that we are ready to loan you a 120 cycle, 1 K.W. 'Simpson' trans- 11060
mitting set, complete; and we now notify you in writing that we have such set complete and ready for you to inspect and test, and that the same is in proper and normal condition for test and investigation. This complete set has all the apparatus, including the transformer, mounted on the panel, the only piece of apparatus, separate from the panel being the 120 cycle generator for supply energy to the transformer and the set on the panel; all exactly as in the case of the like sets sold to the Detroit & Cleveland Navigation Company. The only difference between the transmitter herein mentioned and those installed on the vessels of the Detroit & Cleveland Naviga- 11061
tion Company lies in a slight change in the arrangement and dimensions of the copper ribbon used in the transmitter spiral and helix. The alternating current, 120 cycle dynamo differs from those furnished the Detroit & Cleveland Navigation Company only in the fact that it is provided with a shaft extension. The set herein mentioned is the closest duplicate of the sets furnished the Detroit & Cleveland Navigation Company available. It is from any practical standpoint identical with the Detroit & Cleveland sets mentioned.

"It is not a fact that any particular power

11062 Defendant's Second Letter *In Re* Generator of Simpson Transmitter.

transformer motor generator, or rotary converter, is a constituent part of this 'Simpson' Mercury valve Radio Telegraph Transmitter. The fact is that there is no essential difference in the operation of this radio transmitter, whether it be supplied from a source of power at 60 cycles, or 120 cycles, or 500 cycles. In any such case, the construction and the operation of the transmitting set itself is the same and constitutes our standard set.

"Notwithstanding these facts, we have with considerable difficulty obtained, as stated above,
11063 one of the 120 cycle generators, previously made by us, to enable you to have a sample of the set, not only complete in itself, but also provided with such a 120 cycle source of energy supply, with the object of avoiding any further quibbling.

"Also, in order to afford you the most complete opportunity for fully testing and measuring the 'Simpson' set, we continue to hold for you at the laboratory of the defendant herein, for removal by you, the 60 cycle and the 500 cycle generators previously offered. Owing, however, to congestion in our plant and urgent commercial necessity, we can not hold for you any of the apparatus mentioned herein, longer than five o'clock p. m. today.

11064 "For the same reasons of congestion in our plant and urgent commercial necessity, we will be unable to permit you to retain later than Thursday evening the impulse transmitting set which you removed from the defendant's laboratory this morning.

"In view of the above, we deem it unnecessary for us to be drawn into any further correspondence with you concerning the subject of these tests. and we state finally that our offers stand as above for you to accept or reject at your responsibility. Yours truly, Kilbourne & Clark Manufacturing Company, by E. L. Skeel, its attorney. Service of the above is hereby acknowledged this fourth day of April, 1916."

Defendant's Second Letter *In Re* Generator of Simpson Transmitter. 11065

Now, if the court please, last March the defendant offered in evidence before this court what purported to be a photographic copy of an affidavit of Joseph B. Baker, a witness called by the defendant herein. At that time, if the court please, I called upon the defendant to produce a certified copy of the file-wrapper contents of the Stone patent, of which this affidavit was supposed to form a part, stating to the court that my information was that there was no such affidavits, or any copies of letters annexed thereto on file in Washington in the United States Patent Office. The defendants said that 11066
they would offer that certified copy in evidence, and I ask now if they are prepared to respond to my call.

Mr. Farnsworth: I believe it has been from that time and before that time and that it is now in.

Mr. Betts: Would you please point it out.

Mr. Farnsworth: I cannot go at this moment through all the files, but the file-wrapper of the Stone patent that you refer to was in before you made the suggestion; it was in at the time you made the suggestion and it is now in the case.

The Court: Has it been filed with the clerk? 11067

Mr. Betts: It is not in evidence, if the court please.

The Court: Has it been filed with the clerk?

Mr. Cosgrove. It is No. 767975—I do not find it in the evidence.

Mr. Farnsworth: I will investigate it right away—I assumed of course that it is.

The Court: Is there an affidavit of that nature on file?

Mr. Betts: It is No. 767975, and I call on the defendant to carry out its promise.

11068 Offer of Evidence In Re Austin.

The Court: You can examine the record and see whether it is filed. Proceed.

Mr. Skeel: I will state, if that is not in, it will be put in, if the court please.

Mr. Betts: It should have been in last April.

Mr. Skeel: Are we taking evidence now or arguing the case?

Mr. Betts: I want to be sure that it is in the case.

Mr. Skeel: Have you that record in your hand —will you put it in?

11069 Mr. Betts: I will give it to you if you wish to put it in evidence.

Mr. Skeel: All right, if it is not in we will put it in, and we will pay you for this file wrapper also—if you will give us a bill, whatever it cost, we will pay you. Will you permit me to examine the record first to see whether it is in it?

The Court: You can examine the record and see whether it is in, and you can put that in, and if there is another one in, then this one may be withdrawn. You can mark it as "Defendant's Exhibit No. 65," or if you find the other one, you may withdraw the other one. Proceed.

11070

Offer of Evidence In Re Austin.

Mr. Betts: If the court please, in our notice of examination of the witnesses of the Chaffee tests at the Cruft High Tension Laboratory, which notice has been spread upon the record, we gave notice that we would examine L. W. Austin, and in order that there should not be any controversy or comment hereafter as to the reason why we

 11071

did not so examine him, I would like to prove the facts. It will not take more than a few minutes.

Mr. Skeel: If the court please, I object to this as improper sur-surrebuttal; and we gave the names of three or four people whom we did not examine, and we do not propose to come in and give reasons why we did not examine them—we do not think it is necessary, or that such should be done.

Mr. Betts: I think that comment may be made hereafter that we did not examine Professor Austin, and I think that, as it will only take a few 11072
minutes, we should have the opportunity to explain.

Mr. Skeel: I have no intention to make any comment.

The Court: If a witness were in court and it was a matter upon which the testimony disclosed that he might be a material witness, and he was not called, then it might be necessary to make an explanation.

Mr. Betts: My partner, Mr. Cosgrove, can explain, because he was the one who——

The Court: If the court went into that, why, it would take up unnecessary time. I do not think it 11073
is necessary.

Mr. Betts: May I offer these letters in evidence, without any testimony, merely to show that fact?

Mr. Skeel: I have just told Mr. Hughes that I would make no comment. I do not know what the letters are but I do object to encumbering the record, and I have told Mr. Hughes that I would make no comment on the matter one way or the other. I do not know the reason why he was not called.

11074 Plaintiff's Motion for Surrebuttal Tests of Defendant's Receivers & Rulings.

I know that we called three or four and did not examine them ourselves.

The Court: The only time when the court would consider that, or any court would consider that, would be, if there was a jury present, and the witness' testimony was upon a matter of which the facts disclosed upon the fact were peculiarly within the knowledge of some witness and the witness was within the power of one party to call, possibly, in court it would be proper in court to show
11075 that and then to let the court know the reasons, and to let the jury draw any inference they should desire, but where notice was given that a deposition would be taken and it was not presented, the party is not called upon to explain why it was not done.

Mr. Betts: I wanted to explain that we were unable to get Professor Austin, of the U. S. Naval Radio Telegraphic Laboratory, because the Navy Department would not let him testify. It would only take a few minutes.

The Court: Oh, I do not think it is material.

11076 Plaintiff's Motion for Surrebuttal Tests of Defendant's Receivers & Rulings.

Mr. Betts: Now, if the court please, as indicated the other day, the plaintiff would like to take some answering tests of the receiver tested on July 22 and 24th, which is now in the custody of Mr. Magnusson, and I will say that, barring these tests and any testimony with respect thereto, I do not think that the plaintiff has much, if any, further testimony to offer in reply.

Plaintiff's Motion for Surrebuttal Tests of Defendant's Receivers & Rulings. 11077

Mr. Betts: The tests, if the court please, will be made upon the same two receivers which were marked "A" and "B," and which were produced here when Mr. Marriott and Mr. Magnusson were on the stand last Monday. The tests will be solely and only on those two receivers. As to what the tests will be, I do not know, except that they will be reply tests to the tests which Mr. Thompson made and Mr. Kolster witnessed, and the others witnessed on the 22nd and 24th of July. I cannot define the exact tests, because I do not know. 11078

Mr. Skeel: If the court please, the statement of Mr. Betts that the tests will be reply tests to our tests, seems to me simply a conclusion from something which he stated a few moments ago that he did not know, when he said he did not know what the tests were going to be.

Mr. Betts: I said, the exact tests.

Mr. Skeel: Now, I do not wish to be ultra-technical, and I am not trying to foreclose the plaintiff of any right to show anything that the plaintiff can possibly show in this case, but I do not wish to have a series of tests made here which will open up an entirely new field and might necessi- 11079
tate the defendant coming in and making additional tests, or having testimony to point out what was wrong. I wish to make a statement, because I felt the other day that the court somehow got the idea that in the defendant's tests which were conducted at the University and which were conducted solely for the purpose of answering the plaintiff's preceding receiver tests—I felt that the court got the impression that certain tests had been requested by the plaintiff of us at that time and had been declined.

11080 Plaintiff's Motion for Surrebuttal Tests of Defendant's Receivers & Rulings.

The Court: No, there was no statement of that kind.

Mr. Skeel: Well, I got that impression.

The Court: Just at the moment the thought came to me that I made the suggestion, with relation to these tests, and then afterwards the statement was made at the intermission that entirely cleared the matter. I can appreciate that to have one side make a request and then the other, and intermingle them, that it would be a matter of
11081 confusion and nothing really would be ascertained, and the course that was pursued was the only one practicable.

Mr. Skeel: The only thing that I regretted in regard to those tests was that we could not permit Mr. Weagant to make all the adjustments on the receiver with the condenser, but if we had it would have taken until the present time, probably, and we pursued the same course that the plaintiff pursued; that is, that our engineer made the adjustments and the assessors checked the adjustments. Now, I cannot answer this request of Mr. Betts, and I do not believe that it is fair to throw
11082 the doors wide open and take a series of tests which may or may not be prejudicial to the defendant at this time.

The Court: Let me make the inquiry, now that we are all here; I recall during the examination the other day with relation to these tests; I think when Mr. Thompson was on the stand; I do not remember whether it was in the cross examination or the direct examination, that it was suggested, I think through counsel, that Mr. Weagant stated that the same tests applied to the receivers in the

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present condition as applied to the receivers in the condition they were in when the former tests were made, would produce the same results.

Mr. Skeel: Mr. Betts made that statement.

The Court: I assumed these tests would be for the purpose of demonstrating that thought. That I think would be perfectly proper.

Mr. Betts: I think I stated to the court the other day that the tests we would ask to make would show, as my engineers informed me, that
the same results would be produced with the later 11084
defendant's receiver as had been produced with the former ones—I am using inexact language here, of course—by the later ones I refer to A and B which were here, with the newly constructed coils and the new degrees of coupling.

Mr. Hughes: I think the court has the right idea of the theory on which the tests were to be made.

The Court: Now, I think such a test would be entirely proper. I would not want to have the plaintiff go to work and make or enter new fields—
I do not know whether any fields are open or not 11085
—but I would not want them to offer any different tests or go into any other avenues of experimentation that had not been entered into before, but I think, perhaps, it would be proper to apply to these machines the same tests that were applied—I mean the receivers in the present condition—the same tests that were applied to the other receivers, and then see what the effect of the change was. Now, that is my thought.

Mr. Betts: We are perfectly willing, if your honor please, to limit any tests to these identical machines which were produced before you here on Monday last, the 24th of July.

The Court: Yes.

Mr. Skeel: May, I ask, if the court please, if it is proposed to alter these receivers by the addition of any other condensers, or to substitute or vary any material part of the receiver?

Mr. Betts: We propose merely to use tuning devices with these two particular receivers.

Mr. Skeel: Different ones than the condensers
11087 you have heretofore used?

Mr. Betts: It may be.

Mr. Hughes: I think the court has, generally speaking, defined the purpose of it, and while they used different methods for the purpose of attempting to get different results, counsel should not attempt to tie our hands as to just how we shall attempt to try to show the thing which the court says is a proper subject, and if we have by any results of any test, evidence tending to show that the results obtained in the defendant's tests were inaccurate, why, it would be proper. I do
11088 not see how you can attempt to limit——

Mr. Skeel: I do not want to limit you unduly; I have gotten to the stage where I am so anxious to get through that I am willing to do almost anything to get through with this case; but if we are going to have other tuning devices in here, it is possible that may open the whole thing up.

The Court: I think the same devices should be employed that were employed before; I do not remember just how it was arranged.

Mr. Skeel: I was going to make a suggestion

to the court. It seems to me if Mr. Betts' statement is correct, that is if they say the same things can be done with the receiver in its present condition as was done before, that this is a matter which the assessors themselves could determine, and I think that we might, perhaps, reach a conclusion of this case if specific instructions were given to the assessors to conduct a series of tests and let the assessors make the tests and make the report, without the intervention or interference of either of the parties, and let the assessors 11090
make the adjustments.

The Court: No——

Mr. Betts: There was no limitation asked by us.

The Court: (Continuing)—I would not do that. While the court might, possibly, have been justified originally in doing that, yet I would not want now to restrict one, or limit the active co-operation of one side. I am perfectly willing to have the plaintiff apply the same tests to these receivers in the conditions they are now as were applied formerly.

Mr. Skeel: With the same condensers? 11091

The Court: Well, I think that practically the same implements or instruments should be employed. It may be——

Mr. Skeel: It must be the same condensers, for this reason; don't you see that these tests had to answer those condenser tests?

The Court: I think it should be, perhaps, the same—I don't know enough about this matter of experimentation really to distinguish them.

Mr. Hughes: They were answering tuning

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tests and not condenser tests and they substituted a different coil entirely; it seems to me the thing to be determined when we come into court is, if these men who are experienced have their hands tied when they go out, they might as well be blindfolded. In other words, the defendant said in this court, "Give us leave to take tests and do not put any restrictions on us"; and now they ask to shackle our hands and to put blinders over our eyes.

11093 Mr. Skeel: I beg to differ with Mr. Hughes; our tests were strictly rebuttal tests.

The Court: This controversy won't help any. I think I will leave that in just about that condition; that the tests are to be applied to the receivers—the same tests to be applied to the receivers in the present condition as were applied formerly, and then have the assessors report to the court the conclusions which they find. I think that——

Mr. Betts: We cannot avail ourselves of your honor's ruling if, as we understand your honor's ruling, we can only use the same identical devices
11094 and the same condensers that were employed before.

The Court: Well, I have not said that. I will determine, if there are any different devices used, I will determine later whether this order has really not been carried out.

Mr. Skeel: I will say now, your honor, that if we are limited to the same tuning devices as before that we cannot conduct these tests. The point of the inquiry before was on the old receiver, and was whether or not these circuits were or were

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not tuned or tunable. Now they have changed these circuits, and hence the tuning devices that were used before are not applicable to the new or later modified defendant's receiver.

Mr. Skeel: Now, if the court please, I regret to have to make so many statements, but I must again come back to the proposition that the plaintiff first made the tests on our receivers by adding some foreign element, and they did it by adding condensers and thereby making an untuned receiver into a tuned receiver. Now, I objected at the time, 11096
because I said that this brings a foreign element into the case. The question is not whether our receiver would be better when it is tuned, but whether it is tuned or not. That is what I said was the question in this case, and the plaintiff took the position that if they showed that our receivers would be better if they were not tuned than if they were tuned, they would thereby show that, to some degree, we were taking advantage of the principle of resonance. Now, to combat that contention that those results were attributable to resonance, we simply put the surrebuttal tests in and we specified accurately in advance to what 11097
they would be, particularly and in detail; and those specifications were not accepted. We showed by those surrebuttal tests that the results that plaintiff attributed to resonance should be attributed to coupling. Now we simply answered that, and we did not introduce anything new, and we used exactly the same condensers that they used.

Now, if the court please, I think all these tests are immaterial whenever they try to show whether or not our receiver would be more efficient if it were tuned. That is not the question—"efficiency" is not the question. The question is "Do we tune or don't we tune." If the plaintiff brings in some new and unique tuning device that we never heard about and they are going to show some measure of superiority; I say frankly, that brings a new element into the situation.

Mr. Betts: The new element was brought into the situation by the defendant when it constructed
11099 different coils and constructed a receiver which had different degrees of coupling. Your honor remembers how, instead of the arm swinging backwards and forwards, the coil went in, in the new receiver. Now, hence if we are limited to the exact device used before, it is of no use. We did not limit the other side to——

The Court: They have a device with which they operate the receiver——

Mr. Hughes: But they have used for experiment one with which they did not operate the receiver and now we are called upon to meet that.

11100 The Court: —and I still think that they should be limited to that—that so far as this order is concerned, to the tests that were applied to the other receiver, or to the devices that are regularly employed by the defendant; otherwise it would open up a field that would not assist the court any. That I think is as far as the court should go at this time.

Mr. Hughes: It would not be fair to state my own opinion, because I do not know enough about it for the court to take that as a material guide,

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but certainly as far as I am concerned, we are utterly at sea as to what we can do under the order which the court has made; when we go out we do not know what tests we can make. The defendant's counsel have stated the purpose of the original tests on our part as being for an utterly different purpose than that for which we tested. They contend one thing and we contend another. We made tests for the purpose of establishing our contention and they insist that those tests were made for
a purpose different and to show a different thing, 11102
and we do not agree about that. It is a matter for ultimate argument.

Now, let me explain. Having made our tests for the purpose of showing, not what counsel says but that in point of fact, their receiver as constructed was tuned and try to establish the fact that it was tuned to a wave length between 600 and 300 we used tests for the purpose of showing that fact by establishing that by the use of additional condenser coils we brought the tuning up when we went beyond that wave length, so that it was very greatly improved. Now, we make one deduction
from that. Counsel has here insisted, as he had 11103
repeatedly before, in making another deduction from it. The court must proceed on the theory that we are entitled to make our proof upon our theory of the deduction to be drawn. That evidence was introduced then, not for the purpose of showing that their instrument was not tuned, and that it would be better if it was tuned, but it was offered for the purpose of showing that it was in fact tuned, fixedly tuned within its construction, within a given radius, so that it had relatively a broad

tuning; and that we sought to prove by the methods we employed. Now, they have come in and inserted a new coil within the primary coil instead of having it in juxtaposition to it, or in a position where it would be with its axis at right angles to it and partially removed; they have constructed a coil which goes entirely within the primary coil instead of one which was not entirely within any portion of the lines of inductance or wiring of that coil. That produces a closer
11105 coupling, as has been admitted on all hands—Mr. Marriott says that the two devices operate differently. Now, they have used this kind of coil, not as they contend at all here as a part of their instrument as operated, but they have used it, for what purpose. For the purpose of showing that without the tuned and with a totally untuned instrument by making that addition or change they could get as good results as they do from that receiver which is attacked here and which we say is tuned. Now, what we want to do is to show that these tests which are made do not establish their contention, and we manifestly cannot
11106 use the same appliances and the same methods which were used in the first instance; because our tests in the first instance were applied to their operative receiver; their tests coming after and applying a different mode of operation and different devices to operate, and by it they hoped to prove and seek to prove, and contend that they do prove, that they have a detuned receiver here which gives as good results. Now, we want to show that that is not the case. We want our tests to be confined to their tests and for the purpose

of showing, what we claim, that they do not prove what they contend they prove, namely, that their receiver is an untuned receiver. Now, that is the real contention "Is their receiver tuned or is it not tuned," and we do not want to resort to any new fields of experimentation or novel tests or investigation that is useless. We are not going to take as much time by any means as they have taken in making their tests; we have no idea that we will take as much time as they have taken, and
yet if we make them we want to make such test 11108
as men experienced in the work think will be necessary for the purpose of proving anything. If we must confine ourselves, as the court has said, to the original receiver and to testing that, that is not what we want to do; that is not what you want us to do, and we are not meeting anything which they have introduced. The proper way, it seems to me, is to trust us, as the court trusted them, with our assurance that we are not going to take the time they took in making their tests, but not tying our hands so that when we go out all the time will be taken by objections they make to our pro-
ceeding with particular tests. 11109

The Court: I think the order should stand as indicated. I was about to say before the time Mr. Hughes commenced his argument, that if there is a test that the plaintiff feels is necessary to demonstrate its contention, and such test is made, the court will determine when it is presented whether it really should be received in this connection, and so that the matter may be finally disposed of, if the plaintiff should go beyond the literal provisions of this suggestion of the court in making the demonstrations, and the further demonstrations deemed necessary by the defendant and the defendant desires to make it, I take it that what demonstrations you make should all be completed between now and the time you come in again. I am not going to have any further demonstrations. And that was the purpose of the suggestions, that we do not go beyond anything that may require further experimentation.

Mr. Hughes: It would only be frank to the
11111 court to say that we feel that the court must misunderstand the character of the tests that we desire to make and the purpose of the tests and the reasons that exist in the minds of our expert engineers for making them, or that we misunderstand the court's order. As we understand the court's order, it would be useless for us to make these tests; we would simply be wasting our time and the court's time, because it would not mean anything in addition to what has been already done. It would not be in answer to the new testimony they have given. We hope, therefore, that in view of the last remarks of the court it may be
11112 understood that in making the tests that we conceived to be necessary to make, no attempt will be made to delay us by counsel on the other side——

The Court: Oh, no.

Mr. Hughes: (Continuing.)—so that we are unable to get through—on the ground that it is not within the order of the court, but that they make their objections when we return to court—

The Court: Yes.

Mr. Hughes: (Continuing.)—and offer our testimony.

 11113

The Court: Yes. I will state that this conclusion is predicated somewhat upon the thought that came to me at the time of the examination; and the more I think about it the more impressed is the thought; and that is, that in substance "that the application of the same tests to these receivers would produce the same results as the others." Now, if that is true, then the field of experimentation is limited and very much more so than entering upon a new field of experimentation and which would inject into the matter new and other 11114
elements which had not been developed heretofore.

Mr. Hughes: The court is right to this extent, the same character of tests, but, of necessity, made differently; that is, made with different devices, because it is applied to a different thing.

The Court: Yes, well, you have my idea as to the matter. Now, is that all at this time?

Mr. Betts: Yes, your honor.

The Court: Then we will suspend this trial until Monday at two o'clock. Supposing that the court should get through with the matters which are assigned for the remainder of the week unexpectedly, as sometimes happens, could this 11115
trial be resumed, say, the day after tomorrow, or possibly Saturday?

Mr. Betts: Saturday, I think we could, your honor; as I said a moment ago, I do not think we have much, if any, testimony to offer except the report of these assessors and some tests.

The Court: Then we had better have it go over until Monday afternoon.

Mr. Betts: Would the court have any time on Saturday.

11116

The Court: I do not know; I just suggest that sometimes matters are disposed of very unexpectedly or in much less time than is anticipated, and I just observed that, supposing I got through with the matters that I have assigned for the balance of the week more expeditiously, could this trial be resumed by calling on counsel on both sides? But if this further testimony will take but a short time, I expect Monday afternoon may be better, and then we can definitely arrange our affairs.

11117 Mr. Skeel: If the court please, I am not making a statement to bring this matter up for discussion again, but to get this thing clear in my own mind. Mr. Betts stated the other day, on page 3299 of the record, "I am informed by our engineers that we can show exactly the same results with respect to the new receiver as we have shown in respect to the old." I take that statement to mean that with the tuning device that the plaintiff furnished before and transforming our receiver into a tuned receiver instead of untuned, they can show substantially different results than
11118 Mr. Thompson showed the other day. If they can do it I am perfectly willing to have them do it. On the other hand, if they are now going to transform the receiver into still a third receiver, then that brings a new element into the case entirely. Now, as I understood the court a moment ago, the court has granted permission to the plaintiff to conduct certain tests using the receivers as they now stand and with the same condensers and tuning devices that they used before, and I understand also that plaintiff has informed the court

that if that is the order of the court that they believe there is no use in the plaintiff having any tests; they thereby admitting that they could not do what they said they could do, namely, to produce the same results as before. The court then said if they desired to make any other tests the court would consider the admissibility of them when it came up for hearing. Well, of course, that puts the defendant in rather a difficult position. While I do not wish to throw any impediment in
the way of the plaintiff, but nevertheless if the 11120
plaintiff should produce some new devices that require some study or require a readjustment of the receiver, why, that throws the whole situation up into the air again and I would say at this time that we will not interfere with the plaintiffs, but if the plaintiffs materially alter the situation of this test we will simply content ourselves with so stating. I do not believe that we can keep witnesses here and conduct tests and counter-tests forever.

The Court: No, we don't want that.

Mr. Skeel: I renew my statement of a moment
ago that every test which was made from the time 11121
the new additional elements were added is immaterial to any of the issues in the case.

Mr. Betts: Yet you conducted those tests?

Mr. Skeel: Only after you had put in the condensers and then you changed the situation and we had to change it after you did, and now if you change it again we may have to change it again.

Mr. Farnsworth: When it comes to the argument of the case we will show that the plaintiff was the first one that introduced foreign elements into the plaintiff's receiver.

11122 Plaintiff's Motion for Surrebuttal Tests of Defendant's Receivers & Rulings.

The Court: I will hear you on the argument, but I want to close the arguments on the evening of August the 4th, that is a week from tomorrow.

Mr. Betts: I understood from your honor that you would hear the arguments on the 1st, 2nd, 3rd and 4th.

The Court: Yes.

Mr. Farnsworth: From Tuesday to Friday, inclusive?

The Court: From Tuesday to Friday, inclusive,
11123 sive, yes, and we will suspend the trial of this case until next Monday afternoon at two o'clock.

(Whereupon an adjournment was taken to Monday, July 31, 1916, at the hour of 2 o'clock p. m.)

July 31, 1916, 2 o'clock p. m. Continuation of proceedings pursuant to adjournment, all parties present as at former hearing.

11124 Assessors' Report of Tests.

R. H. Marriott, one of the Assessors, resumes the stand.

Mr. Marriott: Shall I proceed?

Mr. Hughes: Proceed to make your report.

Mr. Skeel: If the court please, before the assessors make any report I wish to make an objection to any report being received upon the tests conducted by the plaintiff on July 29th. I will state these objections and I think I can state the facts thoroughly as to the apparatus used, or if the court prefers it, I can ask Mr. Marriott a few questions to disclose what apparatus was used. used.

The Court: You may make your objection; proceed.

Defendant's Objection to Assessor's Report of Plaintiff's Surrebuttal Tests of Defendant's Receivers and Rulings. 11126

Mr. Skeel: I make my objection, if the court please, for this reason; as I understood the court's order on last Wednesday, the court gave permission to the plaintiff to conduct a series of tests in rebuttal of what the defendant conducted, using the same apparatus and the same tuning devices. The plaintiff at that time said that if the plaintiff were limited to the same tuning devices, they might as well not make any tests, thereby conceding the accuracy of the report made upon the tests of the defendant a few days before. 11127

When, pursuant to notice given to us by the plaintiff, we went to the laboratory on July 29th, we found that the plaintiff intended to utilize a totally different apparatus, a part of which the court sees before it (pointing to apparatus on table). That is, in addition to the condensers which had been employed by the plaintiff before and which were in themselves an element entirely foreign to this receiver, the plaintiff has added the two large inductances or loading coils which you see on the table (pointing).

11128 Defendant's Objection to Assessor's Report of Plaintiff's Surrebuttal Tests of Defendant's Receivers and Rulings.

Now, if the court please, any tests conducted with this additional apparatus furnishes absolutely no evidence that is of any material value in this case. I can say, upon the authority of Mr. Kolster, that the results obtained by the plaintiff with this additional apparatus could be reversed by the defendant if the defendant chose to go ahead and make a further series of tests, and we might go on until the end of time, remodeling and changing this apparatus, and get nowhere. I feel
11129 like saying at this time that none of the tests conducted upon the receiver which have had for their purpose a comparison of this receiver with this receiver modified, have any bearing on this case. If it were the purpose of the plaintiff to show that this receiver is not as efficient in the long wave lengths as it is on the short wave lengths, the only way that that could be done would be by measuring and comparing the intensity of the signals at from three to six hundred meters in its normal operating state, with the intensity of the signals of the same receiver in its same normal operating
11130 state at the higher wave lengths; and that has been done by the defendant in the tests by Mr. Stone, and this has not been contradicted by the plaintiff and they have not been attempted to be modified in any way, shape or form. There has never been any materiality in any tests employing any foreign elements in the receiver.

The second part of the tests constitute a still further modification, and which brings this case almost to the point of the ridiculous, if I might so speak; and that is, the plaintiff in these tests

 11131

took the detector out of the secondary coil of the receiver and transferred it to the antenna.

Mr. Hughes: Do you mean to convey the impression that that was done in all the tests?

Mr. Skeel: No, I said in the second series of tests, Mr. Hughes. There were two series of tests run. The first series employed additional coils and the second series of tests employed the detector in the antenna circuit instead of in the secondary circuit. 11132

Mr. Hughes: If the court please, I would suggest at this time in the record, that counsel again has repeated a practice with which this record is filled—of making statements or drawing deductions as though they were ultimate statements of fact, which are entirely contrary to the facts as we conceive them. He has attempted to put an interpretation upon our tests and our purpose in making those tests, and has stated them as facts, which are entirely foreign to the character of the tests and the purpose of them.

Now, in answer to what counsel has said in the way of objection, I want to say only this; the tests 11133 conducted at the Smith Building employed additional condensers for the purpose of showing —that was the purpose with which they were conducted—that the defendant's receiver was constructed as broadly tuned within the common commercial wave lengths, namely, six hundred and three hundred meters. The methods used were for the purpose of demonstrating that they were so tuned in construction, and that when we got out of the range of the fixed tuning the use of condensers brought into tune; thereby establish-

11134 Defendant's Objection to Assessor's Report of Plaintiff's Surrebuttal Tests of Defendant's Receivers and Rulings.

ing, as we contend, that they were constructed as tuned to the limited wave lengths I have mentioned. Now, the defendant undertook to answer the proof offered by these tests, and the evidence given pursuant to them, by making another series of tests; more particularly did Mr. Thompson claim that it was in answer to certain testimony of Mr. Weagant. In making the last series of tests the defendants did not use their
11135 commercially constructed and utilized receiver, but took out of it entirely the secondary coil, and they made a test with a substituted coil. Now, we did not take anything out of their receiver. We made our tests with their receiver as they built it and sent it out and used it. We used the additional apparatus merely as the means of proving the contention we made, that it was constructed tuned broadly to the commercial wave length. But they made tests, not with their receiver, but with a receiver which had in the secondary circuit an entirely new and different coil—different not only in its construction, but
11136 different in its association with the primary coil. Now, we objected at the time that that proved nothing. The court allowed the evidence to go in, I take it, pursuant to the policy which has been adopted by this court throughout, that this, being an equity case and a long one, the court will not limit, except when all reasonable bounds are transcended, the latitude of parties to introduce such proof as they deem material, but determine its materiality and value ultimately, and then if the court errs in excluding that evidence from its

 11137

consideration either party injured would have the right to a review of its contentions on appeal.

Now, their tests having been allowed in this case, we have taken additional tests made with their new secondary coil, made with their receiver just as they used it the last time, that is, as originally constructed, with the new secondary coil substituted, and we have used again auxiliary apparatus for the purpose of proving that the tests
that they made did not prove what they contended 11138
they had proven.

Now, let me say to your honor that the fact that different apparatus is used is only a method of measuring—of determining the result of the apparatus as used—the instrumentalities employed in making the defendant's tests. If you are going to measure, you must use a measure which accords to the thing to be measured. If you are going to weigh you must use the apothecary or the avoirdupois weight, according to the thing which you are weighing. Now, we will attempt to prove by our testimony that the tests made were
tests made for the purpose of showing and which 11139
show that their tests did not disprove the original evidence offered in the making of our tests at the L. C. Smith Building, and offered by us in support of those tests. That is strictly surrebuttal. They offered theirs as rebuttal to our tests. Theirs are new tests and ours are offered as surrebuttal to their new tests. Now, I make this suggestion to the court, as we are all anxious to conclude this afternoon, that in view of the great length of this trial and of the record, that the

evidence be received and then counsel can be heard in argument on the objections on either side.

Mr. Skeel: If the court please, if it was simply a matter of allowing evidence in, why I would be very glad to waive my objections, but it is a question of when to stop, and it is a question of whether or not I ought to object to this testimony or evidence of tests, or whether, if it goes in, I should ask for further tests, which I plainly say
11141 to the court I do not want to do, because I do not think there is any of them material. Now, there is one statement Mr. Hughes made with which I am in perfect accord; that when you are measuring anything you ought to have a proper measure—you ought to have some unit. Now, that is exactly where the plaintiff and the defendant take issue. The plaintiff, in attempting to measure our receiver tests it with some other, remodeled, laboratory receiver. I say if they want to test the comparative efficiency of our receiver at low or high wave lengths they should test it with itself; with the ordinary measuring instruments, the galvano-
11142 meter; and that they have not done and they dare not do.

Now, in answer to counsel's statement that with the tests we made the other day we remodeled or altered our receiver, I want to call the court's attention to the testimony of the assessors, that the new coil contained exactly the same length and the same size of wire; that the coil was identical with the other one. The only purpose of that coil was simply to permit this coupling switch to be used, so that it could increase the coupling to

 11143

a greater or lesser degree, as was desired. It was simply a matter of degree in this case. Now, in this case the plaintiff not only used that new coil but they hitched onto it this enormous coil (pointing), at one series of wave lengths, and at another series they hitched on this coil (pointing to another coil); and in addition to that they used the same condensers as they used in the Smith Building tests, but in a different order, and not in the
same order at all. Now, I do not want to be 11144
arbitrary in my objections, but I have to do one of two things; either to have additional tests to disprove the conclusions they will draw from these, or else I have to state to your honor what Mr. Kolster has advised me, and that is, that exactly the same results we have shown the other day could be shown with this apparatus, and I do not think any of it is material.

Mr. Hughes: When I suggested the proper way of measuring, my witnesses tell me there is no known way of measuring a receiver such as counsel has suggested; if there had been they
would have employed it themselves. 11145

Mr. Skeel: Mr. Stone did employ it, and the evidence is in this case.

Mr. Hughes: You are mistaken.

The Court: I think, in view of the time that has been taken to receive evidence in this case and the issue involved, as I have stated on several occasions during this trial, that the court should be more liberal in receiving the record of this testimony which is deemed immaterial upon the issues by the respective parties. It is much better for the court to receive testimony that is not material and not consider it, than to exclude something which might be deemed material upon the consideration of the case and then not have it in the record. I think I will receive this, and if this develops into anything that should be met in some way and if you think it necessary, the court will consider whether or not to receive some further testimony. The objection is therefore overruled —the testimony will be received at this time, and an exception will be allowed to the defendant.

11147

Assessor's Report of Plaintiff's Surrebuttal Tests of Defendant's Receivers.

Mr. Hughes: Proceed, Mr. Marriott.

Mr. Marriott: At the engineering building, University of Washington, July 29, 1916.

Test No. 106 was for the purpose of measuring the resistance of the Leeds and Northrup galvanometer as requested, and as used in the defendant's tests July 22 and 24. This measurement disclosed the resistance of the galvanometer to be 430 ohms. The plaintiff's tests followed this measurement
11148 of resistance.

There were present, for the plaintiff's tests, Mr. Hughes, Mr. Cosgrove, Mr. Waterman and Mr. Weagant for the plaintiff; Mr. Skeel, Mr. Thompson and Mr. Kolster for the defendant, and Mr. Magnusson and Mr. Marriott assessors.

Plaintiff's tests began with No. 107. Mr. Weagant made the statement that they would run through a few tests using the coil 1-J, which had been previously employed in the defendant's tests, and comparing the signals when this coil was used

 11149

alone and when the coil was used with additional tuning devices. He also stated that a test with the coil marked 2-J, and formerly used by the defendant, would be made, comparising a comparison between the use of this coil alone and with additional tuning devices. Mr. Weagant stated, in order to expedite the tests, that the assessors make, but one galvanometer reading for each setting.

Mr. Hughes: You mean one at each, or that each of the assessors make one? 11150

Mr. Marriott: Yes, that each of the assessors make but one galvanometer reading for each setting of the instrument.

Mr. Thompson requested that before starting the tests the assessors note that a better reading was always obtained when inductance switch No. 2 of defendant's standard receiver was in the position marked zero on the contact studs; and stated that this indicated something was wrong. Mr. Weagant then remarked:

> "The assessors will also note that if it is not in proper condition, it is at least in the same condition as it was left the other day by the defendants." 11151

Mr. Weagant further stated that he would make the adjustments and ask the assessors to read the galvanometer, and that he might later ask the assessors, providing time was available, to adjust for a maximum. Mr. Skeel at this point said he would write out his objections and give a copy of the written objections to the assessors later. Shall I read these objections, or give them to the stenographer?

11152 Assessor's Report of Plaintiff's Surrebuttal Tests of Defendant's Receivers.

The Court: I think you have made your objections already?

Mr. Skeel: I have stated a part of them, and if this list of objections is simply made a part of the assessors' report, they need not be read at this time, but can be copied into the record. The plaintiff having a copy of these objections, I think.

The Court: If there is no objection, they may be copied and incorporated into the report without reading them.

11153 (Whereupon the objections are copied into the record as follows:)

"Upon Mr. Weagant's statement of the nature and purpose of plaintiff's surrebuttal tests and upon examination of the apparatus, the defendant enters its objection to said tests as follows:

1. The tests are improper because not in rebuttal of defendant's tests, but are tests, employing different devices and receivers than any heretofore seen in this cause. Defendant will insist therefore that the results noted by it on July 22 and 24, are correct and that plaintiff's failure to make tests with the same apparatus as formerly

11154 in an admission of that fact.

2. The tests are in violation of the order of Judge Neterer, limiting the tests to the apparatus heretofore used. We note that in addition to the variable condensers heretofore used the plaintiff is now using large load coils of a variable nature. The condensers are in parallel with these new load coils. These load coils as well as the condensers are entirely foreign to defendant's receiver.

3. That the receiver as now reconstructed by plaintiff is purely a laboratory receiver and is so involved and required such careful and long adjustment as to be commercially impracticable and hence comparative tests with it are useless.

Assessor's Report of Plaintiff's Surrebuttal Tests of Defendant's Receivers. 11155

4. That the variable condensers used by plaintiff in its previous test are now used in different order.

5. That inductance switch No. 2, is out of order and not being used by plaintiff, in spite of defendant's offer to repair.

6. That these tests purport to be new comparative efficiency tests between K & C receiver and a laboratory tuned receiver of a kind never to our knowledge used commercially. That such tests prove nothing as to whether or not defendant's receiver is tuned. That intensity of signals
is the only point plaintiff is noting and not over 11156
all efficiency including quickness of adjustment and the inability of the laboratory receiver to receive signals unless its secondary is exactly and accurately in tune. Defendant also notes that the careful and accurate tuning on this laboratory receiver could not be duplicated in actual practice by an operator on shipboard.

7. The enormous disproportion of inductance now and normally is requested to be noted by assessors.

E. L. Skeel,
Attorney for Defendant."

Mr. Marriott: (Continuing.) In these tests, 11157
examination of the apparatus disclosed that another inductance had been provided for use in the secondary of the receiver. The inductance first shown consisted of a coil, which is here designated as JL, and which coil was connected in series with the secondary, known as 1-J and——

Mr. Hughes: Is that known as J-1?

Mr. Marriott: (Continuing.) 1-J. And a condenser is used across the terminals of these coils when placed in series. The one known as JL, is

11158 Assessor's Report of Plaintiff's Surrebuttal Tests of Defendant's Receivers.

the one I have now in my hand (showing) and it is——

The Court: Is that marked?

Mr. Marriott: That is marked "JL" in pencil on it.

The Court: Mr. Clerk, will you place your identification mark upon this coil JL—mark it as an exhibit.

The Clerk: As a plaintiff or defendant's exhibit?

11159 The Court: Assessors'—it is immaterial, so that it is marked.

Mr. Hughes: He can mark it plaintiff's, I guess.

The Court: Yes, mark it plaintiff's exhibit.

(Inductance coil marked "Plaintiff's Exhibit No. 72.")

The Court: Proceed.

Mr. Marriott: (Continuing.) This coil JL consists of a coil of wire from which a number of connections are made at various points in the coil, and those connections brought to switch points, permitting the connecting in circuits of various
11160 lengths of wire, beginning in the first step, of about one-tenth of the total length of wire, and proceeding to include the total length of wire; thereby cutting in at will more, or cutting out where in series with the secondary of the receiver. The dimensions of this coil JL are as follows: diameter 5 1/10 inches; length of winding 12 inches; number of feet of wire, approximately, 700.

The tests of the plaintiff Nos. 107 to 115, roughly consisted of the following: The tests

 11161

made by the plaintiff using the receiver which has heretofore been known as receiver A with coil 1-J and variable inductance in series with 1-J, the variable inductance being the coil JL and the condenser across both, for certain wave lengths, from one thousand to thirty-six hundred meters, gave greater deflection of the galvanometer when the loading coil JL was used in series with the secondary 1-J, and the condenser used across the two, than when the secondary 1-J, was
used alone. In these tests the galena detector was 11162
used. 1-J used alone as a secondary with or without a condenser across it gave about the same deflection, that is, it gave about the same deflection whether or not the condenser was used across it and the loading coil was not used in series with 1-J on the 450 and 600 meters—about the same deflection occurring on 450 to 600 meters.

Test No. 116 was made with the secondary which has heretofore been known as 2-J, and this showed 2-J as used also gave greater deflection when used with the loading coil condenser than without, and in this case a silicon-antimony detector was used,
which has been heretofore used and which is 11163
known as D-5.

Tests Nos. 117 to 119. In these tests the detector with its stopping condenser and phones was connected first, across the primary of the receiver, and, secondly, across the secondary of the receiver. The connection across the secondary of the receiver being what is usually the normal condition for such a circuit consisting of detector and phones. Two tests were made at 3600 meters and at 1800 meters, in which it was shown that the

11164 Assessor's Report of Plaintiff's Surrebuttal Tests of Defendant's Receivers.

signals were stronger when the detector and its phones and stopping condenser were connected across the primary. At the tests at 600 meters the signals were about the same, whether the detector was connected across the primary or whether it was connected across the secondary.

A report of these tests in tabulation form will be offered for proper copying into the record, and after which tabulation it will be noted that check marks are made in the last column indicating that
11165 the assessors varied the adjustments to ascertain if Mr. Weagant had offered an adjustment for maximum sound.

In the next to the last column references to notes are made, which notes will be read into the record following the tabulated report.

(Here the tabulated report is copied into the record as follows:)

11166

Tabulated Assessor's Report of Plaintiff's Surrebuttal Tests #107 of Defendant's Receivers.

Test Number	Wave Meter Coil	Wave Meter Degrees	Wave Length	Receiver A Inductance Switches 1	2	3	4	Long Wave Switch	Sec. Load Coil	Sec. Condenser	Coupling	Galvanometer Readings Marriott	Magnusson	Series Conden-ser,	Notes	Checking for a Maximum Setting by Telephone
									JL	C_2						
107	A	106.5°	3600	11	0	9	7	2	8	C_2-48°	8¼″	11.1	10.9			
"	"	"	"	11	0	9	7	2	0	Out	10″	2.2	2.2			
108	A	75	3000	11	0	8	8	1	8	26°	7½″	16.6	16.4			
"	"	"	"	11	0	8	8	1	0	Out	10″	5.8	5.9			
109	A	47	2400	11	0	11	7	0	7	27°	9″	16.5	16.3			√
"	"	"	"	11	0	9	7	0	0	Out	9½″	2.7	0.5			√
110	B	93	1800	11	0	9	3	0	3	13°	7½″	11.5	11.4			
"	"	"	"	11	0	9	3	0	0	Out	9½″	8.8	8.9			√
									JL_2							
111	C	134°	1000	9	0	6	0	0	3	16°	4¾″	12.1	12.2		1	
"	"	"	"	9	0	6	0	0	0	Out	7¼″	10.5	10.5			√
112	C	47.5°	600	5	0	3	0	0	Discon-nected	20°	4½″	12.8	13.0		2	
"	"	"	"	5	0	3	0	0	"	Out	5½″	12.0	12.2			√
									JL							
113	A	106.5°	3600	11	0	9	7	2	Out	Out	10″	2.3	2.5		3	√
114	A	75	3000	11	0	8	8	1	Out	Out	10″	3.5	4.4		4	√
										C_3						
115	C	26.5°	450	11	0	3	0	0	Out	15°	4½″	7.1	7.0	98°	5	
"	"	"	"	8	0	3	0	0	Out	Out	4¾″	7.7	7.6	97°		√
										C_2						
116	A	116.5°	3600	11	0	11	6	2	2	18°	6″	13.5	13.3		6	
"	"	"	"	11	0	11	6	2	0	Out	9″	7.0	6.9			√
117	A	106.5°	3600	11	0	11	5	2	......	......	10″	19.0	18.6		7	
"	"	"	"	11	0	11	6	2	......	......	10″	18.5	18.9			√
118	B	93	1800	11	0	7	4	0	......	......	10″	21.0	21.3			√
"	"	"	"	11	0	7	4	0	......	......	10″	17.5	17.3			√
119	C	48°	600	3	0	3	0	0	......	......	3½″	14.4	14.5			
"	"	"	"	3	0	3	0	0	......	......	6″	14.5	14.5			√

11170 Assessor's Report of Plaintiff's Surrebuttal Tests of Defendant's Receivers.

Mr. Marriott: (Continuing.) Note No. 1. The coil JL^2 was used at this point in test No. 111. The coil JL^2 is another loading coil somewhat similar to coil JL, but having different dimensions. The diameter of JL_2 was 3 inches; length of winding 4-7/16 inches, and it contains 235 feet of wire. It is also divided into sections, controlled by a small switch, enabling the number of turns to be varied. In the coil JL_2 the first button is a dead button, and the second button cuts in one section of the
11171 coil, which is different than in the coil JL; as in the coil JL the zero button is a short-circuiting button cutting in no section of the coil, and the first button cuts in one section of the coil.

Note 2 is a note on test No. 112, and it indicates that no loading coil whatsoever was used in series with the secondary; that is, the loading coil being entirely disconnected.

Note 3, is in reference to test No. 113, and is to indicate that test No. 113 was made for the purpose of checking test No. 107. When test No. 107 was made the assessors were not asked to check by varying the position of the switches, to
11172 see if Mr. Weagant offered a maximum, and Mr. Weagant offered this test No. 113 to correct that condition.

Note No. 4, refers to test No. 114, and in that case the assessors were asked to pay particular attention to the adjustments and to see if they in a reasonable time obtained a better maximum than was used by Mr. Weagant, which was done by the assessors to such an extent as was possible by varying the inductance switchings and coupling.

Mr. Hughes: And was there any better maximum found by the assessors?

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Mr. Marriott: No; no better maximum was found by the assessors. That check meant no better maximum was found.

Note No. 5, refers to test No. 115 and to the fact that the series condenser of the defendant's standard receiver was at this point used in the primary of the receiver.

Note No. 6, began with test No. 116, a silicon-antimony detector was used, and the coil known as 2J; the secondary known as 2J was used. At
this point Mr. Kolster asked Mr. Weagant why 11174
the silicon-antimony detector is substituted for the galena detector. Mr. Weagant replied he would not answer at that time, but would do so if questioned in court.

Note No. 7, refers to test No. 117, and it is noted that in this test the galena detector and no condenser in secondary 1J were used with the double throw or double pole switch so arranged that the detector and phones and stopping detector could be connected at will, either across the primary or secondary of the receiver. This was done in tests Nos. 117, 118 and 119; that is, al-
ternate measurements with it first across the 11175
primary and then across the secondary.

At the conclusion of test No. 119 when the plaintiffs stated that that was the end of their series of tests, the apparatus was left in care of Professor Magnusson for delivery to the court today, and Mr. Skeel said that their engineers wanted to make tests, and that the assessors will be notified if the defense wishes them to witness tests on Monday morning. Mr. Cosgrove objected, saying that the assessors were not ordered to make tests further than those of plaintiff, and to report at 2 p. m. Monday. Mr. Skeel said they would ask the assessors to make the tests if made, and would take all responsibility to the court and assessors. Mr. Thompson then asked the assessors to report on the resistance of the galvanometer used in these plaintiff's tests, and to note that the galena detector used in plaintiff's tests was not used in the defendant's tests on July 22 and 24.

Tests Nos. 80 to 105, inclusive, the galena de-
11177 tector was not used in those tests.

Mr. Hughes: Do you mean to say that these last matters occurred in the presence of the plaintiff's representatives?

Mr. Marriott: I could not say whether that one did or not.

Mr. Hughes: I understand they did not. It seems to me that any matters of that kind are not essentially a part of the report, anyway.

Mr. Marriott: The last that I read, the plaintiffs may have been there, but it was about the time of their going away. I do not know whether they were there or not. Mr. Thompson made sev-
11178 eral requests along about that time for things which he wanted us to report on, and—shall I read those or not?

Mr. Hughes: Any of those matters that did not occur in the presence of the plaintiff, if they are to be brought in, it seems to me that that should occur after we have finished our testimony.

Mr. Skeel: I do not know just what it is Mr. Thompson requested, but I do know that the assessors have been requested to make measurements repeatedly throughout these tests, and have

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made them with and without the presence of the parties.

The Court: I think I stated when we suspended last week that any tests that were desired to be made by the respective parties should be made and the matter submitted to the court at this session.

Mr. Hughes: The point I am making is that I am anxious to expedite our matters and am not raising the point as to whether it will be received or not received, but in the proper order that it 11180
should come after we have finished our testimony, if it is material—Mr. Skeel can look into it and find out what he wants of it.

The Court: If this is not part of the plaintiff's tests, and it has not become a part of it, then of course this report should be received later.

Mr. Skeel: We made no tests. The defendant made no tests. What Mr. Marriott is referring to is simply certain measurements or check-ups which the assessors were requested to make.

The Court: In following out these tests?

Mr. Skeel: Yes, sir, on their tests.

The Court: Oh, I think that may go in now— 11181
read it.

Mr. Marriott: I will read here the results which I found, and which will appear otherwise anyway.

The Court: Proceed.

Mr. Marriott: (Continuing.) It was found that coil 1J contained about 100 feet of wire, and coil 2J about 72 feet of wire as first used, and about 66 feet of wire as afterwards altered; the coil JL contained about 700 feet of wire, and that coil JL_2 about 235 feet of wire. That the galvanometer used in plaintiff's tests on the 29th had a resistance of—well, that comes in in the tests that were made this morning.

Mr. Hughes: Were there tests made this morning?

Mr. Marriott: Yes. We had made tests at your request this morning—we had to make tests for these blocking condensers this morning, and for measurements.

11183 Mr. Hughes: Go ahead.

Mr. Marriott: (Continuing.) At 9 A. M., July 31, test No. 120, which consisted of a measurement of the resistance of the galvanometer used by plaintiffs on July 29th. The resistance on this galvanometer was found to be 1420 ohms, and its serial number is 19265. It was manufactured by Leeds & Northrup of Philadelphia.

Test No. 121 was for the purpose of measuring the capacity of the stopping condenser used in receiver A, and that capacity was found to be five-thousandths of a microfarad.

Test No. 122 was for the purpose of ascertain-
11184 ing the capacity of the stopping condenser used in receiver BB, and that capacity was found to be four-thousandth of a microfarad.

That completes the assessors' report.

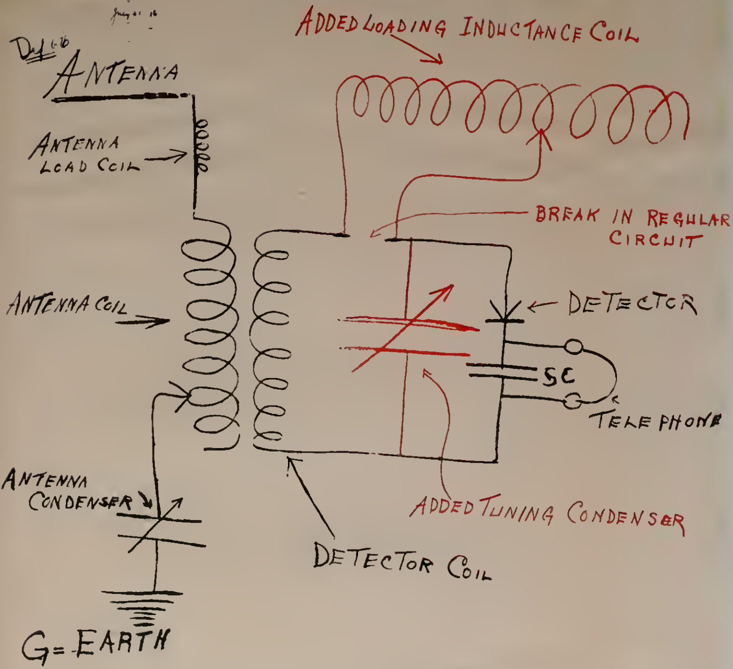
Mr. Skeel: Have you any questions, Mr. Hughes?

Mr. Hughes: I think not.

Mr. Skeel: I have one or two questions.

Q. Mr. Marriott: I have drawn a chart (reproduced opposite) here which I wish to ask you if it correctly

Assessors' Chart.



BLACK = SHOWS REGULAR CIRCUITS OF DEFENDANTS STANDARD RECEIVER

RED = SHOWS TUNING CONDENSERS AND COILS ADDED BY PLAINTIFFS

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 11191

shows the regular circuits of defendant's standard receiver in black, and shows, in red, the added elements which the plaintiff used in the tests of July 29? A. I believe it does.

Q. Will you please just explain to the court just what are the added elements? A. The defendant's standard receiver consists of a primary coil having in series with it a loading coil which is connected with the antenna and the primary coil at its other end being capable of being connected to ground or to a condenser?

The Court: Let me suggest that you mark these 11192
by letters as you go along so that a person can consider the evidence in this connection.

The Witness: We will mark this chart.

The Court: It is the assessors' chart?

Mr. Skeel: As I drew the chart, I will mark it as a defendant's exhibit.

The Court: Very well, let it be marked as Exhibit No. 66.

(Chart marked "Defendant's Exhibit No. 66.")

The Witness: In this chart, defendant's exhibit No. 66, at the top the line marked "antenna"
indicates the antenna as used and that is connected 11193
to the loading coil, which is marked "antenna loading coil," through the primary, which is marked "antenna coil," through a condenser which is marked "antenna condenser," down to the earth, which is marked "G"—equals "earth."

That is defendant's primary of their receiver. And the secondary of the defendant's receiver consists of a single coil which is not in itself variable in the number of turns, and that coil having its ends—having one end connected to a detector, which is marked "detector" in this chart, and from the detector the connection is made to what is known as a stopping condenser, which I will mark "S. C.," and from the stopping condenser a connection is made from the other end of the secondary to the receiver coil. Around the stopping condenser the telephone receivers are connected.

In the tests of July 29 the plaintiffs inserted in the secondary of this receiver a coil the turns of which were variable by means of a small switch.

Q. And that is the coil JL, is it not? A. This coil
11195 being coil JL, or coil JL_2, as the case might be; and JL or JL_2 is indicated on this sketch by the words "Added loading inductance coil." This coil JL, or added loading inductance coil, was connected in between one end of the secondary and the detector or the condenser, I am not sure which way it was connected there.

Mr. Hughes: You mean the stopping condenser?

A. The stopping condenser. It can be either way. And in addition to that, a condenser which is marked here "Added tuning condenser" was connected across from one terminal of the secondary coil to one terminal
11196 of the added loading inductance coil. That is, putting that condenser across the total inductance in the secondary circuit, which total inductance consisted of the normal secondary, plus such loading secondary as was added by using this added loading inductance coil.

Q. (Mr. Skeel.) Now, referring to another chart which I have drawn—

Mr. Hughes: Just one moment. Mr. Marriott, you said, "The normal secondary," you mean the special secondary that was constructed for use, or used in the last of the tests of the defendant, and

 11197

not the one regularly a part of the defendant's receiver, don't you?

A. The word "normal" was, probably, not well chosen. I mean the secondary as the secondary was used by the defendants in their tests on July 22 and 24.

Q. (Mr. Skeel.) I will ask you, Mr. Marriott, if the secondary coil of defendant's standard receiver and defendant's secondary coil J1 is alike in the respect that it is not variable as to time period? A. The secondaries employed, J1, and in their standard receiver, are not 11198
variable as to the number of turns.

Q. Is there any arrangement to cut in or out any of the inductance in those coils? A. There is no arrangement to cut in or out any of the inductance in those coils by switching methods such as is introduced here by this loading coil.

Q. And neither in defendant's standard receiver nor the receiver employed by defendant in its tests on July 22 and 24 was there any variable element either in the inductance coil in any condenser——

Mr. Skeel: Is that a fact?

A. There was, in the defendant's showing, or tests of 11199
secondaries, no means shown for changing the number of turns of wire in the secondary.

Mr. Skeel: And was there, in defendant's receiver—I am not speaking now of the added tuning device, but in the defendant's standard receiver, any variable condenser such as is shown in defendant's exhibit No. 66 in red?

There was no condenser used in defendant's standard receiver across the secondary coil, except where it was used for comparison—comparative methods or measurements.

Q. (Mr. Skeel.) Now, referring to another chart, which I will ask to have marked as defendant's exhibit No. 67 (reproduced opposite), I will ask you if that correctly represents in its two figures, the change made by plaintiff in its tests of July 29th, wherein the detector was transferred from the detector circuit, so-called, to the antenna circuit? A. I believe that represents what was done in the last three tests made by the plaintiff on
11201 July 29th. Do you want me to explain the diagram any?

Q. I don't think so. I simply wanted to have this introduced so that it could be observed.

(Chart received in evidence and marked "Defendant's Exhibit No. 67.")

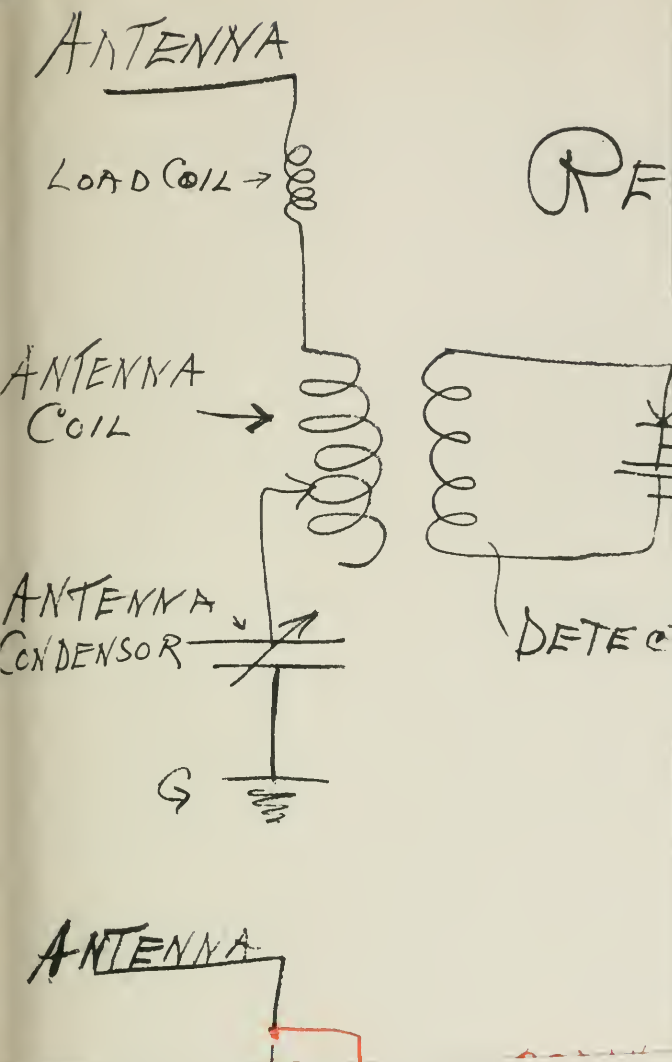
Q. Mr. Marriott, is it not a fact that in defendant's tests on July 22 and 24, Mr. Thompson simply replaced the standard detector coil with a coil having the same amount of wire, but so shaped that a closer coupling or relation of the antenna coil could be had, and that he then applied to this differently shaped coil, the same tests as
11202 were applied by the Marconi engineers to the standard coil in their tests in the L. C. Smith Building?

Q. (Mr. Skeel.) Does that correctly state the facts, Mr. Marriott? A. What was that question, would you please repeat that please?

(Question read to the witness by the stenographer, as follows:)

"Q. Mr. Marriott, is it not a fact that in defendant's tests on July 22 and 24 Mr. Thompson simply replaced the standard detector coil with a coil having the same amount of wire, but so shaped that a closer coupling or relation of the antenna

Assessor


ANTENNA
LOAD COIL
ANTENNA COIL
ANTENNA CONDENSOR
G
RE
DETEC
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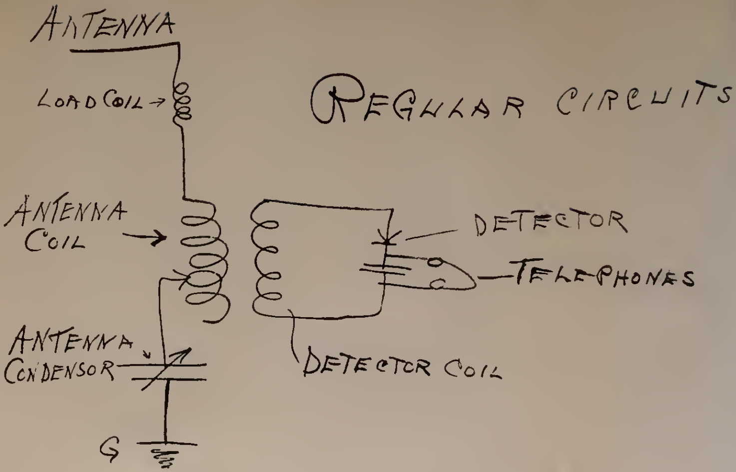
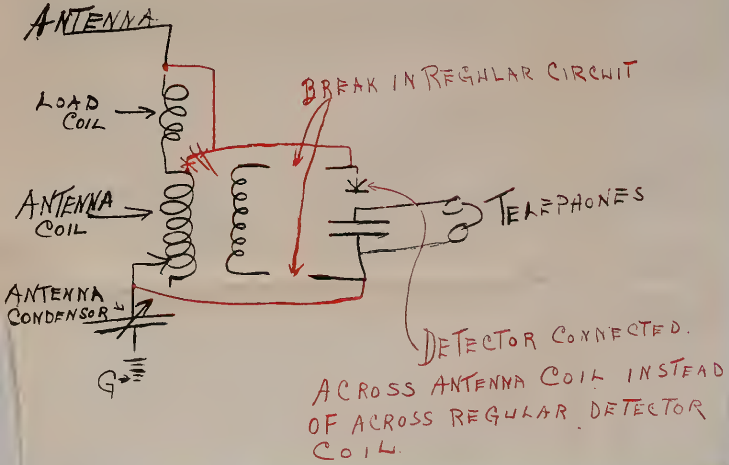
Assessors' Chart.

#67 July 31 '16





 11209

could be had, and that he then applied to this differently shaped coil, the same tests as were applied by the Marconi engineers to the standard coil in their tests in the L. C. Smith Building?

A. As nearly as I can remember, he applied the same class of tests to this coil with which he replaced the standard coil of the standard receiver, and that this coil that he replaced the standard coil by, contained the same length of wire and was made of smaller diameter, so that it could slide in or out of the primary.

Q. Does that finish it? A. I think that covers it. 11210

Q. Now, please state whether or not the Marconi engineers in the plaintiff's tests of July 29 applied to the differently shaped coil, that is coil J1, now in defendant's receiver, the same tests as they applied to the standard coil, when they tested it in the L. C. Smith building? A. No. There was a difference, in that they added a series coil to this secondary, which they did not add to the secondary of the standard coil in the L. C. Smith Building tests, and while they used some of the same condensers used, they did not use the condensers C1 for example, although they used C2 and C3.

Q. (Mr. Skeel.) Who manipulated the instruments and 11211
made the adjustments on July 29th? A. Mr. Weagant manipulated the instruments and made the adjustments, except, as noted by checks in the tabulated report, that the assessors did vary the inductance and coupling of the secondary, or varied the coupling and inductance of the primary to ascertain whether or not Mr. Weagant had given a maximum.

Q. (Mr. Hughes.) Are you not mistaken in the connection of the red circuit, in the lower one of these coils, exhibit No. 67—in other words, does it connect to the antenna coil? A. It should connect at this point (showing).

Mr. Skeel: Please make the change so that we will have it right; mark it with a red pencil.

A. It should connect at the point above the point marked "loading coil," and I will now cross out the former connection.

Mr. Skeel: That is defendant's exhibit No. 67?

11213 Q. (Mr. Hughes.) In respect to the use of the condenser, referring to exhibit No. 66, you say that there is no variation of the condenser used by the defendant in making its tests; as a matter of fact, the tests made by the defendant were—half of them, without any condenser at all, were they not? A. Well, you will have to read back to me what I said to you before.

Q. Well, all that I want is merely to clear it up. What I want to show is, you only mean to say that on the condenser there was used—there was more provision for variability in the coil that they used; but you do not mean to say that they did not make their tests without the use
11214 of the secondary coil at all; do you? A. They used a secondary coil, only that coil was not constructed to have its number of turns varied.

Q. In one-half of their tests they used a condenser on the coil, and in the other half they did not, isn't that the case? A. In certain tests, where they were making comparisons to ascertain whether that secondary coil worked better with and without a condenser; they used a condenser across, and then they did not use it across, and compared the two results.

Mr. Hughes: That is all.

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The Court: Is that all by this witness?
Mr. Skeel: That is all at present.
(Witness excused.)

PLAINTIFF'S SURREBUTTAL EVIDENCE.

R. A. Weagant, recalled on behalf of plaintiff in surrebuttal, testified as follows:

Q. (Mr. Hughes.) Referring, now, Mr. Weagant, to your tests on the defendant's receiver at the Smith Build- 11216
ing before the assessors; did you change or alter the defendant's receiver, as Mr. Thompson has claimed in his testimony, in making those tests? A. I certainly did not. The receiver was used exactly in its condition as I received it in.

Mr. Skeel: I do not want any inference to get into the record in respect to Mr. Thompson's claiming that he altered the receiver.

Mr. Hughes: His testimony will speak for itself.

Mr. Skeel: At this time I wish to disclaim any statements of this kind by Mr. Thompson. I claim that he never made it. 11217

The Court: Proceed.

Q. (Mr. Hughes.) Mr. Weagant, will you state to the court the nature and object and purpose of the tests made on July 29 at the University of Washington in the presence of the assessors, and which tests have been reported to the court? A. Those tests were made for the purpose of showing that the tests which Mr. Thompson had made with the specially constructed tuner did not demonstrate or prove the fact which they claimed that they did, and furthermore, to show that even with this modified form of receiver the secondary system was already tuned to the range of commercial wave lengths; it was built in that way, and to show furthermore, that when tuning devices were applied to this new secondary system that, in so far as any other ranges of wave lengths are concerned, the operation of the receiver was very materially improved in so far as the strength of signal or loudness of response was concerned.

Q. In what respect was the secondary of the defendant's receiver altered or changed for the purposes of the
11219 tests last made by the defendant? A. Well, the coil was constructed in an entirely different shape and in such a shape that it could be inserted within the primary coil. Now, that results in two things, first of all, a very much closer coupling is obtainable under those conditions than is obtainable in the standard form of receiver. In addition to that, this adds a new variable element to the receiver; in other words, it adds a variable capacity. Those two coils when used in that way, one inside the other, have a very considerable capacity, and that capacity is adjustable by the distance which the secondary coil is inserted into the primary coil. So that, in effect, there are secured two simultaneous adjustments, one of coupling and one
11220 of capacity.

Q. Is this close coupling, obtained by the use of this substituted secondary which was used by the defendants in their last tests, attainable on any other standard receiver as made and used and sold by the defendant? A. No, it is not. The standard receiver is so constructed that only a comparatively loose coupling is attainable.

Q. Will you explain what the tests made by the defendant with these substituted secondary coils proved, if anything? A. Well, they failed entirely to prove anything, insofar as any useful thing in wireless telegraphy

is concerned. They simply showed that when the real advantage of a secondary system in a receiver, namely, the ability to get selectivity, was thrown away by coupling very, very closely, that the device would give a louder response than when the same secondary system was tuned with devices, which were not adapted for giving maximum sound or responses, but which were adapted for giving maximum selectivity. So that, when Mr. Thompson made his tests and adjustments he adjusted the modified coil when used alone in such a way as would give a maximum response and maximum signal strength; whereas, when
he purported to be tuning that circuit he adjusted it, not 11222
for maximum signal strength, but for maximum selectivity.

There are two objects always sought in tuning a secondary circuit of a receiver system; one may be maximum signal and the other may be selectivity; and the desirable thing depends on the conditions which you are working under; but in practical work, the real main object of a secondary system is to secure selectivity. Now, Mr. Thompson sacrificed that entirely in the construction and adjustment that he used, and then he failed to use it—when he compared the intensity of the signals of the coil
alone, and the whole circuit at once—he failed to use these 11223
devices which are well known to every radio engineer as being the necessary thing to use when you are attempting to get louder signals, namely, the inductance coil as well as the condenser—both inductance and capacity are necessary for tuning, and they may be used in various proportions, and it happens that in the secondary system of the receiver apparatus that the proportions which give the greatest signal, or at least that arrangement which gives the greatest response is when the inductance is a maximum, and the capacity a minimum.

Q. Mr. Thompson claimed, I believe, in his testimony

11224 that these tests showed that the untuned receiver was better than a tuned one; what would you say as to that testimony of Mr. Thompson? A. That statement is not warranted by any facts of the tests. In so far as the intensity of signal is concerned, it is not warranted, as shown by our own tests on the 29th. Furthermore, even though under all conditions, assuming that the circuit were tuned in the best possible way and that the single coil used alone gave louder responses, no one would ever use a tuner in that condition of adjustment in practical work; for the simple reason that the tuner under that
11225 adjustment loses all its ability to exclude interfering signals. Now, in the last three tests which were made at the University of Washington on the 29th, I showed that by connecting the detector directly into the antenna circuit and dispensing with the secondary coil entirely, that the strength of the signal was as great or greater than it was when the special coils which Mr. Thompson constructed were used.

Now, it is obvious from that that if this mere intensity of signal were the desired object, that the designer in building the receiver set would not go to the expense and trouble of building the second coil, nor would he put on the operator the additional trouble and work of making
11226 an extra adjustment.

A. (Witness having read to him his previous answer continuing)—as anything else.

Consequently it is perfectly obvious that in so far as it has any bearing on the real usefulness of the receiver, Mr. Thompson's tests, even if they had been properly conducted, would have failed to support his contention.

Q. Now, referring to the tests which were made by you on the 29th; state briefly to the court what they

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proved, and the reasons why the conclusions you reach are drawn? A. Well, they proved, first of all, that this new coil was already tuned—had tuning built into it in the commercial range of wave lengths. Tests were made from 3600 meters down to 425 meters, each time a comparison was made with or without extra tuning devices, and it was found that at all places except the 600 and 425 meters, which are the commercial ranges of wave lengths, the operation of the device was materially improved, but that at those two wave lengths it was not possible with any additional devices to increase the effectiveness. So
that the only possible conclusion from that result is that 11228
already the secondary circuit was as much tuned as it possibly could be.

Q. In the arrangement employed by Mr. Thompson in his last tests, what was the use of the secondary coil that he employed; that is in substance? A. In so far as the practical working is concerned, that coil might much better have been left out of the apparatus altogether. My last three tests on the 29th showed that the signal strength was just as good or better, connecting directly to the primary coil. And I know from other tests and experience that in the matter of selectivity the very close coupled secondary coils such as he had used, is substan-
tially the same thing as connecting the detector directly 11229
to the primary.

Q. Why is it customary to adjust the secondary coil so as to be loosely coupled, or to construct it so that it will give loose coupling? A. In order to work through into interferences and to secure selectivity. If the coils are loosely coupled the energy transmitted from the primary to the secondary is slower and the selectivity or sharpness of tune is greater.

Q. Is the defendant's standard receiver as constructed

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and used commercially, so constructed for the purpose of securing selectivity? A. Yes, it is so constructed.

Q. When the secondary circuit of the receiver is loosely coupled to the primary what is to be done to secure a satisfactory loudness of the responses? A. When that condition obtains, then it is necessary to tune the secondary circuit in order to get sufficient loudness. As soon as the coupling is made loose, then the energy falls off and the transfer becomes too slow, and too little energy gets across, unless you tune that secondary circuit. So that in order to make an operative receiver it is necessary
11231 to have, first of all, a loose coupling for selectivity, and then the tuning in order to get sufficient energy across to be practically useful.

Q. Mr. Weagant, Mr. Thompson gave some testimony, and I think made some tests with reference to the period of 200 meters; did he show by any of his tests that such a wave length would be useful for commercial purposes, the circuit having a natural period of 200 meters? A. No, he certainly did not. He only made one test with the circuit having a period of 200 meters or under. Now, in making that test he did not use the form of construction used in the standard receiver, and about which I had been talking when I made my comments relative to the 200 meter
11232 circuit. All he did show in those tests was that with the especially constructed coil coupled on a very close coupling, it was possible to get a louder response than you did with one of their own receivers—their own standard receivers, at 3000 meters. Well, that hardly can mean anything, in view of the fact that we have already shown in our Smith Building tests that the Kilbourne & Clark standard receiver at 3000 meters is anything but a standard of efficiency; showing that the 200 meter coil or circuit, were equal to the Kilbourne & Clark receiver at 3000, does not establish any fact at all, because it previously has

been proved that at that wave length the receiver was very inefficient. Furthermore, in making the tests in that particular way, the real, necessary purpose of the secondary coil, namely, the securing of selectivity, was entirely thrown away.

Q. The Assessor's report to the court shows that at the commencement of your tests on Saturday, July 29th, Mr. Thompson called attention to the fact that switch No. 2 of the tuning coil was out of order; what have you to say in respect to that? A. Simply that it had been out of order during the tests that Mr. Thompson and Mr. Kolster had made. Mr. Kolster had made the statement dur- 11234
ing that test, and he repeated it at some time during our tests on the 29th, to the effect that it was out of order when I used it. It was in the same condition it was in when Mr. Thompson used it, whatever that may be. It was my purpose to make the tests upon the apparatus in the same condition as the tests were made by Mr. Thompson.

Q. That was your intention, you say? A. Yes, that was my intention. That was the only way I could make any comparative tests.

Q. I believe that it is claimed that it was out of order when test No 2 was obtained; what difference would it make in your tests, the result of your tests, and the object 11235
and purpose of your tests, whether switch No. 2 was working or not? A. I do not think it would make any difference. I set it during all the tests so that it was at the zero point, so that any turns of inductance which it controls were not in use, so that any defect which may have been in it was not in actual circuit during the tests.

Q. Mr. Weagant, you heard the testimony of Dr. Zenneck and Mr. Simpson, and you heard or read the testimony of Mr. Stone and Mr. Simons given respecting the tests that were taken at the Cruft Laboratory? A. Yes, I have.

Q. And you have noted the criticisms made by these witnesses in respect to certain conditions which have obtained, as they claim, during those tests. One of them relates to the condition of the gap. Will you tell the court what the facts were in respect to that. A. Yes; in the first place up to July 3rd those gaps had not been opened from the time they were received by us from the Kilbourne & Clark Company. They were used only a moderate amount in the Marconi laboratory, and then they
11237 were used by Dr. Chaffee for some considerable length of time, and up to July 3rd there had been no indication of any trouble or irregularity in their operation.

Q. Were you there and did you observe them on July 1st and 2nd? A. Yes, I did.

Q. They were working all right during the tests made by Dr. Chaffee on the 1st and 2nd of July? A. Yes, quite a lot of tests were made.

Q. State what else. A. On July 3 evidence of trouble was discovered, and the gaps were opened. The plates were cleaned out. That is, the bumps on the sparkling surface were turned off in the lathe, which is the normal
11238 method employed by every one in repairing a burnt spark plate. We have, in our service, hundreds of those gaps, and that is the procedure we follow always when they have been burned. The gaps were put together again. In the process of putting them together the gaskets, a couple of the gaskets were damaged some, but after we put them together again we sealed them with sealing wax so as to keep them as nearly airtight as possible. The criticism has been made that the gaps were too fresh in the tests. As a matter of fact they were seasoned.

Q. How were they seasoned? A. Dr. Chaffee ran the

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apparatus on the morning of July 4th for, I should say, at least two hours.

Q. Before he began the series of tests which he photographed on the afternoon of the 4th? A. Yes, before he began the tests at all, and the set had an amount of operation which was more than we are accustomed to give our regular sets before sending them out into service, what we call a little seasoning. Now, when these gaps were opened again I noted them particularly. They were in very good condition, and I noted especially that there was evidence that the gaps had been, so far as this airtight condition was concerned, rather better than it was deliv- 11240
ered to us originally.

Q. Did you notice anything wrong with the apparent operation of the spark gap during the tests in the afternoon of July 4th, in the light of your experience as an engineer and constructor and user of such apparatus? A. Nothing whatever. As far as the gaps were concerned they were performing in perfectly normal and satisfactory manner.

Q. Some suggestion and criticism has been made by some of these witnesses respecting the measuring instruments that were used by Dr. Chaffee; what have you to say on that? A. Simply this; insofar as the antenna 11241
ammeter was concerned we had the very best type of ammeter, namely, an instrument for the purpose of measuring that instrument during the preliminary adjustment, which was accidentally burned out, so for that reason we could not use it during the official tests, and had to borrow one from the laboratory, but insofar as the current in the circuit is concerned I will say that I had measured it with this instrument with the set in exactly the same condition of adjustment, and that I found that there was always as much or more current in the antenna during all of the

tests than there was in the tests reported by Mr. Kolster in the Bureau of Standards reports.

Q. It has been claimed by some of these witnesses that the tone or note was not always clear; what have you to say about that? A. Well, I listened to the note a good deal, and I would not call it a first-class note. It was the sort of note though, that you most generally hear in service. The pure, ideal note will, however, often be obtained in the laboratory, but seldom is in service. I think the character of note that we obtained during those tests was about that of a moderately good note, speaking
11243 from the ordinary commercial practical point of view.

Q. Would the particular clearness of the note have anything to do with the result of the tests made by Dr. Chaffee with the aid of the Braun tube? A. No.

Q. Something has been said about the brushing of the condenser; what have you to say about that? A. Well, simply that the brushing had no effect on the tests at all. We made various measurements and looked into it generally to see if everything was all right in that circuit, and we were perfectly certain that it was.

Q. The criticism has been made by at least some of these witnesses to the effect that Dr. Chaffee gave no measurement of the decrement; what have you to say
11244 about that? A. Simply that Dr. Chaffee did make the measurement.

Mr. Skeel: You say he did or did not?

A. He did. He took resonance curves, and I think the information is available.

Q. What is it? A. I think the results of his observations in that respect are available.

Q. What have you to say of the methods of measurement employed by Dr. Chaffee, to which some criticism has been offered? A. Those methods were not only ex-

 11245

ceedingly good, but constituted a very remarkable piece of scientific work. None of the criticisms made are directed at the fundamentals of the methods at all. At best they are merely slight superficial objections.

Q. Did Dr. Chaffee explain in your presence to all these witnesses how he arranged all this apparatus? A. Yes, he did. He went over all the details, just what he did after he took the resonance curve, and showed them where he located the apparatus when he made it.

Q. There has been some criticism offered by some witnesses because of the use of the generator employed; what have you to say about that? A. Well, that gener- 11246
ator was a perfectly good, normal 500 cycle machine for use of this kind, and on account of the fact that we provided it with a variable reactance it mattered not what its characteristics were or what those of the transformer were, because it was entirely susceptible of meeting any condition that might exist in the set. I believe the comment was made that the transformer should have 200 or 220 volts applied to it, and that this machine was a 120 volt machine. As a matter of fact, the name-plate on that machine very distinctly said, "220 volts," and the generator was capable of delivering that or higher pressure.

Q. It has been suggested by some of these witnesses,
or intimated, that Dr. Chaffee chose the time for taking 11247
his photographs; what reason, if any, was given for taking photographs at particular times only, for getting better results? A. He had to do that from the very nature of the tests. In order to get the photograph of this moving spot of light and to get the photograph distinct, so they would show distinctly, it was necessary that the moving spot should traverse the same path over and over again twenty or thirty thousand times during the exposure. Now, in order to get it to do that it was necessary to get the spot operating with just as clear a note and

11248 just as steadily as possible. If Dr. Chaffee could have obtained an absolutely pure note it would have been the ideal condition from the standpoint of measurement, but that could not be obtained with the set. It was sometimes clear and sometimes rough, and it behaved very much like any set of that kind will in that respect, and the result was that Dr. Chaffee had to watch for those periods when it was clear in order to get his photographs. That had nothing to do with the question of whether or not there were oscillations there, but it was of use to determine how many, because in the absence of a clear and distinct photo-
11249 graph a blur would merely show that there were oscillations, but would require distinct points or lines to tell how many.

Q. Mr. Weagant, it has been suggested by some witnesses that there were or may have been no current in the inside plates of the Braun tube, as used by Dr. Chaffee; what do you say in that respect? A. It is my recollection that Dr. Chaffee explained very clearly that he had made the necessary tests to determine whether or not that was true, and he had found that there were no odd currents.

Q. From your observation of these tests of the apparatus, what do you say as to whether there are, in fact,
11250 in the Simpson mercury valve transmitter two different circuits? A. Well, there are most certainly two circuits in the Simpson mercury valve transmitter, and this is proven conclusively by the fact that in order to get any satisfactory or proper operation of the Simpson set you have to make adjustments in two circuits. If it were a single circuit transmitter then you would adjust a single circuit and obtain proper operation, but you actually have to adjust two circuits, namely, that circuit which has been designated as the antenna circuit, and the circuit which has been described as the spark or trigger circuit.

 11251

Furthermore, you have to adjust those two circuits to a very definite relation to each other, namely, until their time periods are the same, or until they are in resonance with each other, and that fact has been proven by the tests of Mr. Kolster, reported in the Bureau of Standards report, by Dr. Chaffee's tests with the Braun tube, and by my own tests in the laboratory at Aldine, where I found that the set would not work unless those two circuits were both adjusted to substantially the same time period.

Q. Mr. Thompson in defining an impulse transmitter
said that it was one in which the oscillations last longer 11252
in the antenna circuit than in the primary circuit; I will ask you now, in the light of your experience, if you know of any transmitter in radio use in which the oscillations do not last longer in the antenna circuit than in the primary circuit? A. I certainly do not. Every transmitter that I have ever seen has many more oscillations in the antenna circuit than it has in the spark or trigger circuit.

Q. With the spark gap taken out of the antenna circuit and put in the second circuit, associated with the antenna circuit, what do you have to do to get the energy which it controls back into the antenna circuit? A. You
have to do two things. You have to couple the circuit 11253
containing the spark gap to the antenna circuit, and you have to tune it to the same period as the antenna circuit.

Q. You have constructed the exact apparatus shown in the Marconi patent, have you? A. I have, yes.

Q. Were there more oscillations in the trigger circuit or in the antenna circuit in that apparatus in use? A. There were many more oscillations in the antenna circuit than there were in the trigger circuit.

Q. To get a single wave radiated——

(Question repeated.)

11254

A. I did not finish the answer. Yes, as I recall it, there are four arrangements described in the patent exactly. The radiated wave was a pure wave, of single frequency. I might say that the apparatus as built was built from exactly the dimensions and specifications given in the patent.

Q. Is the energy originally or initially in the antenna circuit or in the primary circuit, the spark gap circuit, in the Simpson mercury valve transmitter? A. The oscillating energy is initially in the trigger circuit. That is shown very clearly in all the tests which have been made
11255 on that point, and the fact that it is transferred from that circuit to the antenna circuit at a later stage is shown.

Q. By the way, Mr. Weagant, there is one matter of criticism in the testimony of the defendant which I have not alluded to; that is the use of a dummy antenna by Dr. Chaffee. What reason was there for using the dummy antenna, or what reasons, if there were more than one? A. Well, a number of reasons; first of all it is a common practice to do it in that way, and it was employed by the defendant; in the next place, there was no actual antenna of the right size available, all the antenna at the college were very much too big. The natural periods were too long, and, furthermore, we could not have conducted tests
11256 of that kind, on actual antenna without getting into trouble with the Navy Department or the Department of Commerce and Labor. We would have been violating the law.

Q. Mr. Weagant, the Assessors' report showed that Dr. Kolster raised a question as to why you did not use the silicon antimony as the detector, and that you refused, or qualifiedly refused; what are the facts about that, and why? A. The reason why I used that particular form of detector I think was clearly brought out in the tests in the L. C. Smith Building. I stated at that time, and the

same reason holds good now, that it was impossible to make galvanometer tests with the silicon antimony or arsenic detector, because it did not stay in any permanent adjustment, and in the Smith Building tests I made a long series of tests with the Assessors using the telephone only and the galvanometer to show substantially the same results were obtained with the silicon antimony detector as with the galena detector. Now, the detector which I used at the University of Washington was a galena detector and was exactly the same thing, the same sort of thing, the same crystal as used in the tests in the L. C. Smith Building, and, as I showed, gave the same sort of 11258
results as the silicon arsenic, or silicon antimony detector.

Q. Why did you use the galena detector? A. So that I could make proper galvanometer measurements. The other detector will not stay in adjustment long enough.

Q. I noticed Mr. Skeel in the objections that he has submitted to the court to the Assessors' test No. 5, objects on the ground that the inductance of switch No. 2 was out of order, in spite of the defendant's offer to repair; was there any offer made to repair? A. None that I heard of.

Mr. Hughes: That is all.

11259

CROSS EXAMINATION.

Q. (Mr. Skeel.) Mr. Weagant, you took the generator furnished you by the Kilbourne & Clark Company back to Massachusetts, did you not? A. I took it back to Aldine.

Q. And you did not use this generator at any of those tests? A. No.

Q. I understand you to say that Dr. Chaffee took some resonance curves which were available. A. Yes, I did.

Q. Do you know whether those resonance curves were taken at any of the tests on July 3 or July 4? A. I do not recollect that they were, but I am not positive. I do not know to a certainty just when they were taken. I know they were taken in that same condition and adjustment.

Q. Were they taken at any time when the Braun tube photographs were taken which have been introduced in evidence in this case? A. It is my impression they were, but I am not certain.

Q. Please specify the date upon which those curves
11261 were taken, and by whom? A. Well, they were taken by Dr. Chaffee.

Q. In whose presence? A. That I do not know either.

Q. Were you present? A. No, I was not.

Q. You say those curves are available; has any reference been heretofore made to those curves that you speak of in this case? A. Not as far as I recollect, no.

Q. And so far as you know those resonance curves have not heretofore been furnished to the defendant or its attorneys or witnesses? A. I do not know that they have.

Q. And you heard Dr. Zenneck testify the other day to the fact of the absence of those curves, and made no
11262 offer to give data on the curves? A. Well, I do not recollect that Dr. Zenneck testified in just that way. I understood him to say that he would like to know what the resonance curves showed. I think that is the reason they are here today.

Q. Did you or did you not, while Dr. Zenneck was on the stand, or at any other time heretofore, offer to produce any resonance curves to us? A. I did not, no.

Q. You have referred in your testimony to the generator and you said that it was a 220 volt generator, how do you know that? A. Why, I am rather familiar with the machine. It has that labeled on the name-plate, if I recollect right.

Q. Did you measure the voltage? A. I did not measure it during the tests, but that happened to be a machine that I am somewhat familiar with.

Q. Do you know as a fact that 220 volts was used on the day of the tests? A. I did not measure the voltage on the day of the tests.

Q. I am asking you as a fact whether it was or not? A. I say I do not know, I have answered that.

Q. You said a few minutes ago it was a 220 volt gen-
erator; do you wish the inference to be drawn that 220 11264
volts were used on that date? A. Why, I am not intending that any particular inference shall be drawn from that, except that the machine used was capable of delivering the 220 volts. That is all that was necessary as far as the tests were concerned.

Q. On page 9 of his direct testimony Dr. Chaffee says as follows: "This transformer is connected to a 500 cycle, 125 volt generator," do you agree or disagree with Dr. Chaffee? A. I think probably Dr. Chaffee made a slip in referring to the rating of the generator. I happen to know to a certainty on that particular point. I know that it was a 220 volt machine.

Q. Do you recall what the name-plate on that gener- 11265
ator said as to voltage. A. It is my recollection that it said 110-220 volts.

Q. What instrument was used to show the amount of current in the primary of the transformer? A. I do not recollect whether any instrument was used or not.

Q. Just before the recess reference was made to my objection on the ground that inductance switch No. 2 of the receiver was broken and was not used in the tests; I will ask you to tell the court whether or not during the progress of those tests I handed to your counsel, and whether or not you read my objections, which contained that one? A. I remember you handing the objection, but I think I did not read it until after the tests were all through.

Q. I will ask you to state whether or not you did not sit down on the bench and before noon read the list of objections through which contained this statement that we objected, and that you continued in the tests despite our offer to repair. A. I do not remember that I did that, Mr. Skeel.

A. (Continuing.) I do not remember those objections,
11267 that is, personally reading them, until the tests were finished. I am not at all certain about that, because I do not remember the incident with any particular distinctness.

Q. Referring to the three condensers which were used in the L. C. Smith Building receiver tests of the plaintiff, and a part of which were used in the University tests of the plaintiff, I will ask you to state whether or not by the use of those condensers, added to the secondary coil, the defendant's receiver, the time period of that receiver could be accurately tuned to wave-lengths of ranges from 600 to 3600 meters? A. Yes, I think so.

11268 Q. Could the secondary of the receiver be just as accurately tuned by the use of those condensers as by the use of the condensers and the coil? A. Just as accurately, yes, most certainly.

Q. Why did you use the coil in addition to the condensers? A. I think I have explained that.

Q. Do it again? A. I think I explained it very carefully and fully in my direct examination. I used it simply in order to employ tuning in an efficient and proper manner. You might tune these two circuits, one circuit in one manner and the other in another manner, and not get any benefit from it. Tuning is not magic. It is merely one of several vital things. When that circuit is tuned with the condenser alone and with that particular coil, that substitute coil, it is not tuned in the most effective way from the standpoint of loudness of signal. That is why I used another equally common method of tuning.

Q. Did I understand you to say that both variable coils and variable condensers are necessary for efficient tuning? A. No, I do not think I said that.

Q. Did I understand you to say that changing the coupling changes the wave-length of the detector circuit of the defendant's receiver? A. I think a question of that
kind is impossible to answer. 11270

Q. Did you make the statement in direct examination that changing the degree of coupling changes the wave-length of the detector circuit of the defendant's receiver? A. I do not think so. I said nothing about changing of coupling, and the wave-length of the secondary circuit, at least I do not recollect that I did.

Q. Does the changing of coupling change the capacity of the detector circuit? A. I think it does, yes.

Q. Does the change of capacity change the wave-length? A. Yes, I think it does.

Q. To what extent can you change the wave-length of the defendant's detector circuit by increasing or decreas-
ing the coupling, to the extent that the coupling was in- 11271
creased or decreased in the plaintiff's receiver tests of July 29th? A. I do not know fully to what extent that can be carried. I noted very carefully and positively that in using the condenser to tune with, the condenser alone, that as the coupling between the coils was made closer, the amount of capacity necessary for the maximum indication, which in the absence of any other information I will assume as tuning, became less, as the coupling became closer. The effect of coupling alone would be the reverse of that effect. It tends to neutralize somewhat

11272

the secondary circuit, and if it were present alone it would then require larger capacity as the coupling became closer, but the observation which I made showed that it required less capacity as the coupling became closer, thus indicating and showing very distinctly that a change in capacity was taking place simultaneously with the change in coupling.

Mr. Hughes: I could not hear quite all of the witness's answer; may I ask if you will testify in respect to the standard receiver when its secondary coil is used commercially or with a substitute coil?

11273

A. This refers to the substitute arrangement.

Q. Can you refer to or cite any authority, Mr. Weagant, to the effect that changing the coupling to the extent ordinarily required in commercial radio work, changes the time period of the detector circuit to a substantial degree? A. Well, the ordinary authorities——

Q. (Interrupting.) Just answer yes or no, whether you can or cannot. A. I do not know of any authority which deals with that problem in its entirety at all.

Mr. Skeel: That is all.

11274 (Witness excused.)

W. W. O'Farrell, Recalled as a witness on behalf of Plaintiff, in Surrebuttal, testified as follows:

Q. (Mr. Hughes) Give your full name. A. W. W. O'Farrell.

Q. Mr. O'Farrell, you have already testified that you are a wireless operator. A. Yes.

Q. How much experience have you had? A. Since 1908

 11275

I have been in the wireless business, and electrical business, together.

Q. In what positions, or what service? A. In the installing and operation.

Q. In what appointments—I mean, in whose employment do you work—part of the time for the Government? A. Yes, I was employed by the Government between six and seven years.

Q. Are you familiar with the Telefunken and Marconi instruments? A. Yes, sir, I am familiar with both.

Q. Are you familiar with the defendant's receivers?
A. Yes. 11276

Objections to Evidence, Offer and Rulings.

Mr. Skeel: If the court please, at this time, I object to any testimony on the part of this witness, for the reason that this witness is not included in the list of witnesses heretofore given by Mr. Betts the other day when we each stipulated to the court the names of the witnesses that were to be produced by the parties from that time on. Now, I make this statement, if the court please, not because this witness can testify to anything that the
defendant is afraid of, because I know precisely 11277
to what he will testify, but simply to warn the court if this witness testifies it opens up a new field in this case, and if Mr. Hughes wishes to make at this time a statement of his testimony I will state my objections in full. I make this statement for the reason that the court will recall that at the close of the testimony on April 29th the plaintiff rested so far as the receiver and the Thompson transmitter were concerned.

11278

Mr. Hughes: No, the receiver was a matter of dispute here; you have given testimony in regard to the receiver.

Mr. Skeel: Just a minute—and the court said that the plaintiff would not be permitted to put in any more testimony on those matters unless they made a showing that would, in substantial effect, entitle them to a new trial. That is the statement the court made at that time.

Mr. Hughes: The question I am about to ask is one responding directly to the testimony given
11279 by the defendant during last week by Dr. Zenneck and others. It appears in some of the testimony of the witnesses called, operators called. They called at least two operators who testified in regard to the receivers. I do not think I am right when I speak of Dr. Zenneck, because I do not think he testified in regard to the receivers. The court made no such order in respect to the receiver at all. On the contrary, it has been the subject of testimony very largely.

Mr. Skeel: I cannot make my objection more specific unless counsel will make a statement as to the purpose for which he has called this witness.

11280 The Court: What have you to say about the understanding that the names of witnesses to be called should be submitted to the other side?

Mr. Hughes: My statement is that counsel's statement made in open court dispenses with all such necessity. He knew perfectly what this witness would testify to. What is the necessity of furnishing the names of witnesses? My reason for calling this witness is that he has only recently been available to us. We had no means by which we could obtain his testimony. The testimony can be disposed of very quickly.

The Court: If this testimony is submitted it would open up a new field.

Mr. Hughes: I will put my question and then the court can rule, and then we will see whether they are on the field that is opened.

Mr. Skeel: On page 2888 (print p. 2923, Vol. 4) of the record the court made this statement, the plaintiff was then closing its rebuttal case, and the court said:

"Now, I want to close this apparatus now, and 11282
if any further testimony is to be presented a showing must be made of almost sufficient to grant a new trial."

Mr. Hughes: You only read a part of it, that with relation to the transmitter and not the receiver. I just asked him about the receiver. You have introduced testimony since that time about the receiver, about how far they could hear and all that, and I want to ask a question or two that would take half of the time these objections would
take. 11283

Mr. Skeel: About the receivers?

Mr. Hughes: Yes, what I was asking about.

Mr. Skeel: Is it limited to that?

Mr. Hughes: No, when I get to another question I will let you know.

Mr. Skeel: I object to all of it.

The Court: Propound your question. I do not propose to open up any new field that may perhaps involve some period of time.

11284 W. W. O'Farrell—Recalled—Direct.

Q. You testified that you have operated the so-called Thompson receiver, manufactured and used by the Kilbourne & Clark Company, the defendants? A. I have.

Q. The standard receiver? A. The standard receiver.

Q. I will ask you to state what is the effect as to whether you can receive with that receiver effectively under ordinary commercial conditions wave-lengths of over 1600 meters, or thereabouts?

Mr. Skeel: Just a minute, if the court please. I object to that as being improper surrebuttal, and
11285 also for the reason that it calls for the conclusion of the witness, he has asked the witness a question as an expert, involving an opinion.

Mr. Hughes: He has shown eight years' knowledge. I can qualify him adequately on that question, if the court thinks there is nothing in it.

The Court: Is the other objection waived?

Mr. Skeel: It is not waived for this reason, if the court please—if I am permitted to put in evidence in rebuttal I will waive it, but not otherwise.

The Court: Objection sustained. Exception noted.

11286

Q. Do you know the steamship Norwood? A. Yes, sir.

Q. That is equipped with the Simpson transmitter, is it? A. I was informed that it was so equipped.

Q. Have you communicated recently with the Norwood? A. Yes.

Q. What do you say as to the note of the Norwood——

Mr. Skeel: I object to that.

Q. (Continuing) The note of the transmitter of the Norwood.

Mr. Skeel: I object to that as incompetent, irrelevant and immaterial in surrebuttal.

Mr. Hughes: If the court please, Mr. Simpson gave testimony about the note of the apparatus as it was used during the tests. I want to show that the note of the Norwood corresponds exactly with what Mr. Simpson testified was the note of the apparatus at Harvard University, the Cruft Laboratory. That is the purpose of it.

Mr. Skeel: Do I in return have to go out and get other operators in answer to this testimony?

The Court: Objection sustained. Exception 11288
noted.

Mr. Hughes: One minute, I want to make my position clear. They have had an opportunity to show the character of the note. This is the only opportunity we have had to rebut that testimony. I wish the court to fully understand it, if this is the ruling of the court I desire to make my offer; the plaintiff offers to prove by this witness now on the witness stand that he is engaged with the Norwood, has been recently, and that he is familiar with the character of the note given out by its transmitter; that it is a ragged or uneven note,
sometimes distinct, sometimes uncertain, and 11289
more or less indistinct, and that the character of the note is such as was described by Mr. Simpson as the note heard by him in the use of the Simpson apparatus at the Cruft Laboratory, in which testimony he stated that it was not their ordinary commercial note in commercial use, and further to show that at the time the witness communicated with the Norwood the Norwood was in service, was in transit as a steamship, and was being operated by its operator. Is the offer denied?

11290 W. W. O'Farrell—Recalled—Direct.

The Court: Tender declined; note an exception. I will state in this connection that I did not understand the witness to say that the apparatus or appliance with which this steamship was furnished——

Mr. Hughes (Interrupting): He did testify, and I offer to prove beyond that, that it was supplied with the Simpson transmitter.

The Court: Well, ask him.

Mr. Hughes: He has already testified to that.

The Court: He said he was advised.

11291

Q. How were you advised? A. When I arrived in Seattle I asked one of the operators at the Seattle station what the system was on the Norwood, and he informed me that the Marconi set either had been taken off,—or she did not have the Marconi set, and that the Kilbourne & Clark 500 cycle Simpson mercury valve transmitter set had been placed aboard the Norwood, and I believe that was her first trip with that set.

Mr. Hughes: I further offer to prove by calling Mr. Wolff and the production of the Government license record in the Radio Bureau downstairs in

11292 this building, that the Norwood is equipped with a Simpson mercury valve transmitter, and was at the time referred to by the witness as having communicated with her.

Mr. Skeel: What is the date fixed?

Mr. Hughes: Within the last month.

Mr. Skeel: I admit that the Norwood was equipped with the Simpson mercury valve transmitter during the last month. I admit that fact, and I still renew my objection.

 11293

Q. State the character of note which you heard from the operation of the Simpson mercury valve transmitter on the steamer Norwood, and describe it fully. A. The note when I first started to communicate with the Norwood was very good and clear, and it would work that way for, oh, some seconds or minutes, as is the case in operating, and then later break. I would say it sounded a great deal like the rotary synchronous sets. Then later it would clear up and be pretty good again, would be pretty clear, and then it would break again.

Q. Have you worked as an operator on vessels on the Pacific Ocean using the Thompson transmitter? 11294

Mr. Skeel: Just a minute, I object to that as incompetent and immaterial at this stage of the proceedings, and improper surrebuttal.

The Court: We are getting further and further away, it seems to me.

Mr. Hughes: If the court please, the plaintiff had no knowledge until within a few days of the facts that it expects to prove by this witness, and the plaintiff was unable to obtain and use this witness until today, just having learned what knowledge this witness possessed. The plaintiff desires to prove by this witness that the primary 11295
circuit of the Thompson transmitter was so constructed and provided with such arrangements as to enable it to modify or change its wave-lengths, and thus alter and adjust or tune its primary circuit to the secondary circuit, so as to tune from 600 to 300 meters. I merely make that general statement of the scope and purpose of this testimony so that the court may understand that it is a matter which is involved in this case, and which is the primary question in the controversy between the plaintiff and the defendant over the Thompson transmitter, namely, whether the Thompson

transmitter employed a system of tuning its primary and secondary circuits, a system of tuned circuits, covered by the Marconi patent We have proven the fact of it being constructed so as to be tuned for 600 meter wave-lengths, and we are now offering to prove that it was so constructed and arranged that by a slight change it could be tuned to 300 meters, or for commercial wave-length, and this evidence is evidence that we had not knowledge of before. I will show to the court before the court's ruling on this matter, that the de-
11297 fendant has had possession of its own apparatus, its own equipment, controlled its use on various ships, and the evidence that we have offered heretofore has shown the court the difficulty which we have encountered in attempting to secure evidence as to the precise condition and mode of operation, and nature of construction and use of the Thompson transmitter. This evidence we have been able to acquire now for the first time, and it is material evidence, it is evidence that relates to one of the vital issues, the vital issue so far as concerns the Thompson transmitter and it is for that reason that the testimony is offered at
11298 this time. We have not been able to present it sooner and the defendant admits it is not surprised by the testimony of this witness; in other words, it has known what the facts are.

Mr. Skeel: If the court please, I am very glad to have Mr. Hughes make the statement that the evidence which they now propose to introduce was not available to them before. I do not believe that Mr. Hughes knows the full facts in regard to this matter, but I am satisfied that his client does, and I think we have an issue here which will perhaps test one of the many issues in this case.

 11299

Mr. Hughes: I do not get that, Mr. Skeel.

Mr. Skeel: (Continuing.) Which will test one of the many issues in this case. You made a statement, I am going to make a statement.

Mr. Hughes: The court will take your word for the reading of the testimony.

Mr. Skeel: I do not want the court to take my word.

Mr. Hughes: I am willing.

Mr. Skeel: At the bottom of page 1412, the last line at the bottom:

"Q. What is the fact, Mr. Thompson, about 11300
transmitters, in which there was a variable condenser; I mean of the defendant Kilbourne & Clark Manufacturing Company, installed temporarily, will you please state the facts in regard to that?

Mr. Betts: I object to that as immaterial.

The Court: I will let him answer.

A. Why, certain sets, I believe, altogether four transmitters of the impulse type, of the Kilbourne & Clark Company were made so that condenser could be varied.

(Question read).

The Court: I do not think we care to go into
that if they are not in issue here. It is a matter 11301
entirely outside.

Mr. Skeel: I just want the court to remember I offered to show the facts in regard to that."

Now, if the court please, the plaintiff knew about these four transmitters, the plaintiff knew that those four transmitters had what they call split condensers, which were on just temporarily at the request of the Government inspector, who was a new inspector and did not understand the situation, and if the plaintiff wished to introduce

that matter in evidence the plaintiff should have
done so in the prima facie case. The plaintiff did
not do so. The defendant, pursuing its policy
throughout this case, of giving the court full in-
formation, therefore offered to put in that evi-
dence so that the court might have full information
here in regard to everything. Mr. Betts objected
and the court sustained the objection, and I
directed the court's attention at that time to the
fact that I wanted him to remember that I had
offered to prove that in evidence. Now, if the
11303 court please, the evidence which would be
presented is not anything that would be material
to this case, because it did not constitute the
standard Thompson impulse transmitter. It was
a temporary proposition which Mr. Greaves did
not know about. Mr. Hughes made the statement
that the plaintiff did not know of this matter and
did not have the evidence available until this
time. I want the court to know that this witness
now on the stand has been a witness in this case
last March and last April, and I also want the
court to know that if this evidence is admitted we
11304 would ask to have Mr. Irwin, the former Super-
indendent of the Marconi Company, testify to the
fact that this identical information had been in
the possession of the Marconi Company from a
date prior to October 1st, 1915. In other words,
they have had this evidence in their possession
all that time through the means of four affidavits
taken in this city before a Notary Public in the
law offices of Peters & Powell, that formerly re-
presented the plaintiff in this case. Now, we
might go on forever, if the court please, trying

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new issues in this case. I do not think this is material. It is a matter we have attempted to explain, and the plaintiff knew we could explain, and they refused to put it in, and they purposely held it for the last thing for the purpose of throwing some doubt in the court's mind as to the credibility of the witnesses for the defendant. I do not think that it is necessary or advisable to throw this case open and make it necessary for the defendant to call half a dozen other witnesses to go into this further proposition.

Mr. Hughes: Do I understand you to admit 11306
that you used at least four of your sets prior to the beginning of our action, variable condensers of the primary circuit tuned to 300 meters?

Mr. Skeel: I admit nothing of the kind, Mr. Hughes, I admit nothing of the kind. I admit that I offered to show the facts in regard to this matter, and the plaintiff has objected.

Mr. Hughes: The difficulty between counsel and ourselves, is that their offer to show the facts does not accord with our view of the facts, and what I have just asked counsel to admit shows 11307
that he does not accord with what we expect to prove, and clearly prove by this witness. Now, I want to say that such statements have been made repeatedly throughout the record in this case. We have not taken the time or the occasion to resent them, as I think they deserved to be resented, but when counsel states that we reserved this testimony to the end he states it badly to the court, and he is in effect telling the court that the statement I made to the court is untrue. We had no knowledge of the facts we now expect to prove.

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We had no testimony available by which we could prove the facts that we now expect to prove until today. This is the first meeting of the court at which we had the means to prove these facts. We expect to show not only that they had available condensers, but after the beginning of this action they changed their apparatus so as to cut off the means of varying. We shall contend that previous to the bringing of our action they had their apparatus so constructed as to be capable of tuning to the 300 meter wave-length, it being constructed also to be tuned to the 600 meter wave-
11309 length.

The Court: I think, in view of the record in this case, that the offer should be declined. An exception may be noted. Otherwise, it would be opening up new matter that I do not think the court would be justified in receiving, in view of the record. Note an exception.

Mr. Hughes: In order that the rights of the plaintiff may be preserved of record, I now offer to prove by the witness now on the witness stand that while in the Government service he examined the steamship Dora, equipped with a Thompson
11310 transmitter, that subsequently, prior to November, 1915, he became radio operator on the tug Tyee, equipped with a Thompson transmitter, that both of those vessels contained in their primary circuits two banks of condensers in series with a cover, openings and provision for use of switch plugs so that one bank could be cut out, thereby diminishing the capacity of the condenser one-half; that this arrangement was provided in the set for the purpose of changing the wave-length for use with the 300 meter wave-length, and for

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the purpose of tuning to the 300 meter wave-length. I further propose to show by this witness that in the latter part of November, after he had returned to the dock, he was absent at home for a period of about three hours, and he found on his return that the cover had been removed from the condenser box, and a different and new cover substituted without the plugs, so as to cover up or conceal the fact that the apparatus was so arranged and adjusted originally as to be susceptible of tuning to the 300 meter wave-length. I also propose to show by this witness that he has 11312
examined the Pioneer and that it was originally constructed in like manner with the Tyee, that is, with the Thompson apparatus so constructed that the capacity of the condenser could be reduced one-half by the use of plugs, as described, and that subsequently those covers were taken off and new covers, different in color, were put on the condenser finally. In order to make my offer more complete, I offer to place upon the witness stand Mr. Weagant, chief engineer of the company, who has been present throughout this case, and to prove by him that he has made every effort to ascertain whether or not the defendant company 11313
did not in the use of its Thompson transmitter have a means of changing the capacity of the primary circuit for the purpose of adjusting to a 300 meter wave length, and that the first knowledge that came to him was on the last preceding day of the session of this court, at which time the witness recited the fact to Mr. Weagant in my presence, and likewise explained that he was engaged in preparing a ship to embark, and that it would be impossible for him to come that afternoon to

court, for which reason he was brought here at the first available opportunity.

Mr. Skeel: If the court please, I desire to have filed with the records of this case this affidavit from the former superintendent of the Marconi Company, in view of the fact that counsel's statement in regard to the evidence is substantially a motion for a new trial. I want this affidavit to show that this information has been in the hands of the plaintiff company ever since October last, nearly one year ago.

11315 Mr. Hughes: I do not understand that the court makes an issue of the offer. If the court is about to allow the offer to be proven that is another question.

Mr. Skeel: I wish the record to show that I have evidence at hand to show that the plaintiff had that evidence in its possession.

Mr. Hughes: Counsel has no such evidence at hand. I make my offer and I wish a ruling.

The Court: I think, in view of the record in this case, as I referred to it a moment ago, that including the testimony which was taken in this case on the 7th of April, the objection made to
11316 the testimony at that time, and the offer which is made now, and the order of the court when the suspension was taken, prior to the resumption of this trial on the 18th of July, that the offer should be declined.

Mr. Hughes: I ask an exception. I will call Mr. Waterman.

Mr. Skeel: Just a minute, I want to cross-examine this witness.

Mr. Hughes: There is only one matter on which testimony was given.

Mr. Skeel: Yes.

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CROSS EXAMINATION.

Q. (Mr. Skeel) Mr. O'Farrell, it is the duty of an operator, is it not, to keep a log of his voyage, and to report any unusual or poor conditions. A. Yes, in a general way.

Q. Will you please produce the log wherein you show this poor note on the steamer Norwood. A. I do not know as I even made a note of working with the steamer Norwood in the log.

> Mr. Hughes: Do you mean to say that it is his duty to keep a log of the operation of other vessels 11318
> than his own?
>
> Mr. Skeel: Yes.

Q. Now, Mr. O'Farrell, when did you leave the employ of the Kilbourne & Clark Manufacturing Company? A. I never was employed by the Kilbourne & Clark Manufacturing Company except for a short period last winter.

Q. And when was that? A. It was some time—I do not know the exact date—it was the time of the big snow, along in January, I think.

Q. Of recent months you have been in the employ of
what company? A. Of the Puget Sound Tugboat Com- 11319
pany, and the Pacific-Alaska Navigation Company, and the Alaska Steamship Company.

Q. And those companies all employ the apparatus of the Kilbourne & Clark Manufacturing Company. A. They do, yes.

Q. And you were in the employ of the last-named company until July 26th, were you not, last Wednesday? A. I believe it was the 26th.

Q. What steamship were you employed on? A. On the steamship Jefferson.

Q. And you had signed to go on a trip on July 26th at 5:00 p. m. A. Yes.

Q. You tendered your resignation at 1:55 p. m., did you? A. I do not know what hour it was.

Q. What was your salary as operator on the steamship Jefferson? A. I had the salary of operator and assistant freight clerk, and also mail clerk, $75 a month.

Q. $75 a month and board? A. And board, yes.

Q. You are now employed by the Marconi Company at $100 a month and board, are you not? A. No, there was no board or anything else when I went to work for
11321 the Marconi Company. I did not understand I was to get board, although if I do I will be very much satisfied. I did not have that understanding that I was to get my board.

Q. When did you enter the employ of the Marconi Company last? A. The same day that I tendered my resignation to the Alaska Steamship Company.

Q. Who offered you the position? A. Mr. Weagant offered me the position.

Q. And did you talk to Mr. Weagant and Mr. Waterman both in regard to the position? A. No, I only talked with Mr. Weagant.

Q. Did you endeavor to get Mr. Lipke to also accept
11322 a position with the Marconi Company and testify as you have done? A. I believe I told Mr. Lipke that there was a position open there for a good operator that had any experience, that he could get a position regardless of his testimony or what company he worked for before.

Q. Did you tell Mr. Lipke you believed in looking out for yourself? A. I may have passed that remark. I do not know whether I did or not.

Mr. Skeel: That is all.
Mr. Hughes: That is all.

(Witness excused.)

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F. N. Waterman, Recalled as a witness on behalf of Plaintiff, in Rebuttal, testified as follows:

Q. (Mr. Hughes) Mr. Thompson, I believe, has made the statement in the course of his testimony that the defendant's receiver was changed during the tests conducted by the plaintiff before the Assessors at the Smith Building, which occurred last April, I believe; what have you to say about that? A. The statement has been made a number of times in my hearing, and is entirely incorrect. No change whatever in the apparatus of the defendant's receiver was made in the Smith Building tests. 11324
On the contrary, that apparatus was tested exactly as the defendant constructed it.

Q. Did you witness the tests conducted by Mr. Thompson on the 20th and 22nd of this month at the University of Washington? A. I did.

Q. What did those tests show, just explain to the court? A. As regards the defendant's receiver they showed nothing at all, because the defendant's receiver was not used, but a receiver was used having a coil substituted, which has been marked by the Assessors "1J", which was substituted for the regular standard coil, a difference being that the regular standard coil can only be put against the primary coil, while the coil "1J" 11325
goes completely inside of it. Mr. Thompson used this coil "1J" in some of his tests, and certain others of his tests he used a coil which I believe has been marked "2J"' similarly constructed except as to the number of turns, and the spacing of the turns upon the surface of the spool.

Q. Mr. Thompson asserted in his testimony that those tests conducted by him showed that an untuned circuit was more efficient or better than a tuned circuit; what do you say as to the correctness of that? A. Mr. Thomp-

son is entirely mistaken. There was no foundation for any such conclusion in his tests. The reasons why I make that statement are as follows: The coils which Mr. Thompson used he used in such a position as would destroy any opportunity for selective receiving whatever. The coil which the defendant's receiver normally has is a coil which is intended to have always a measure of selective reception. By selective reception I mean this: the need of anything elaborate in the way of a receiver began when more than one signal began to be sent at a time. If there was never but one signal at a time being
11327 sent the tests conducted by Mr. Weagant the other day showed that the defendants would do much better, or would at least do as well to have no secondary coil at all. The purpose of a 2 coil receiver is to enable a reasonable degree of selectivity to be obtained, and having gotten that then to obtain a sufficient strength of signal. Mr. Thompson made the criterion in making his statement, merely loudness of signal, and on that criterion the defendant needs no secondary at all. As Mr. Weagant's tests show, they would do better to have a detector simply connected around the primary coils. They actually have a secondary which is never capable of such an association with the primary as would give the so-
11328 called tight coupling. Now, there are several ways of tuning. I think, perhaps, I can make that clearest if I draw a diagram (reproduced opposite, F. A. W. 30). The Marconi patent in suit, for example, shows the four ways, which have all been illustrated in these tests. I will draw only the secondary circuit, in order to have the simplest diagram. The apparatus may be constructed initially tuned, which means that a coil is wound, a stopping condenser and a detector selected, such that the the effective capacity around the whole circuit and the effective inductance of the circuit determine a certain

Fig 1

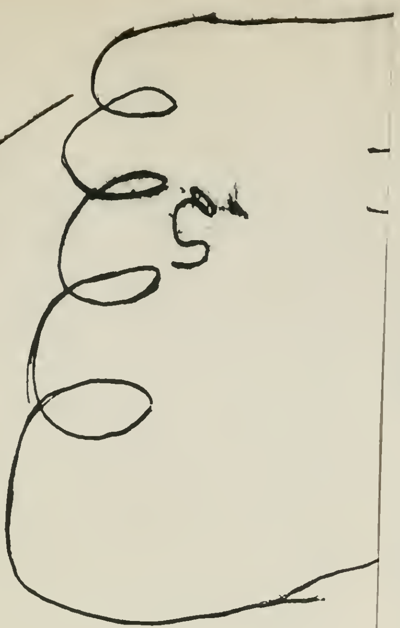


Fig 2

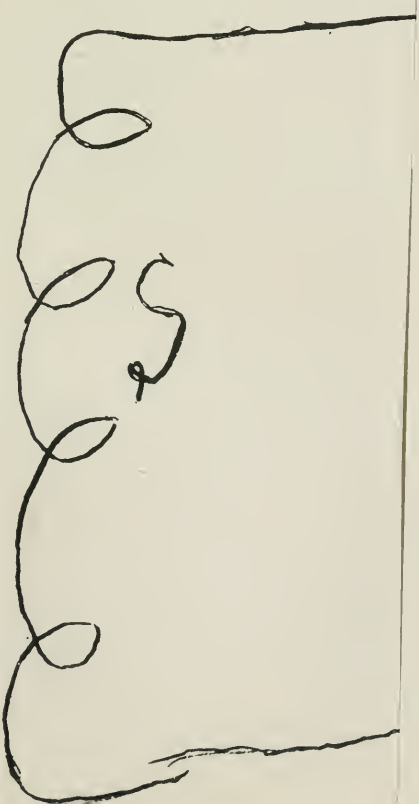


Fig 1

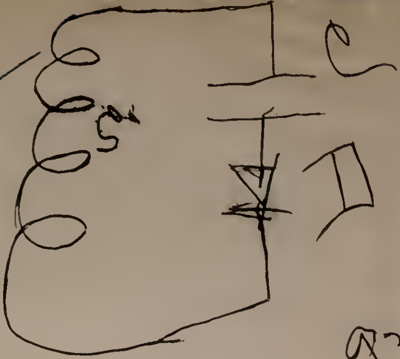


Fig 2

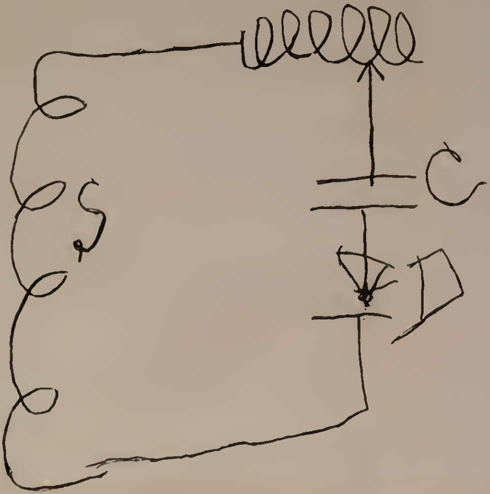
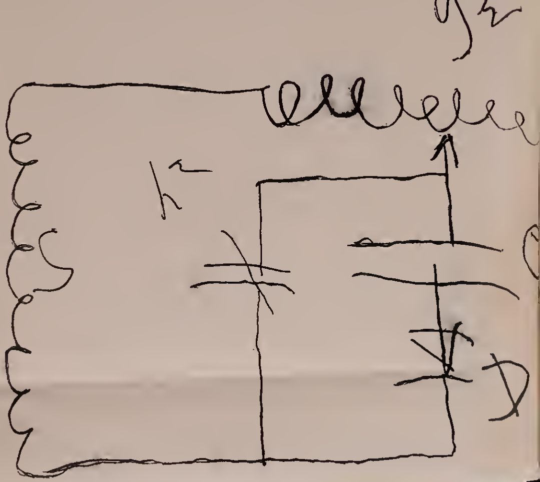


Fig 3

Fig 4



natural time period. That is the normal defendant's receiver circuit.

The description in the patent also includes an arrangement in which there is the soil S, the condenser C, the detector D, as before, and also a coil, if I remember the letter of reference right, g^2, in the drawing of the patent. That is a circuit which is tunable to various wave-lengths. Fig. 1 of my sketch is built tuned to a single wave-length. Figure 2 is tunable to a varying range of wave-length, determined by the size of the coil g^2.

The description in the patent also includes another 11336
mode of tuning (illustrating Fig. 3) in which a condenser h^1 is connected across the coil S, and this method of tuning, therefore, has a range from the natural wave-length to which the circuit without the condenser is tuned up to the maximum that will be given by the capacity of the condenser.

The patent finally illustrates the apparatus for tuning wherein the coil S has inserted in series with it the coil g^2, as in my Figure 2, and the two have connected across their terminals the condenser h^2, which alone is shown in my Figure 3 (reproduced opposite), which is shown connected across coil S alone, and the circuit also 11337
includes, as before, the condenser c, and the detector d. Now, those different kinds of tuning have different properties, and are used under different circumstances. I have pointed out to the court in my former testimony that if one wanted to receive a certain narrow range of wave-length nothing is equal to my Figure 1 construction, with its coil and secondary circuit tuned to the wave-length it is desired to receive. If we want to have a variable circuit, and if the object of tuning is the loudest possible signal, then Figure 2 is the mode used. If we are using the more usual—if we are trying to take

the more usual advantage of tuning, that is, the selective property of it, and do not care about loudness, then Fig. 3 is the method. If we want the tuning to be very exact, very loud, and be able to get selective or loudness, then we use the complete arrangement which is shown in Figure 4.

I have already read the passage in the patent, page 1, lines 15 to 19, which makes it one of the primary objects of the invention that oscillations or electric waves from a transmitting station may be localized——

The Court: There is no use reading over again
11339 what has already been read.

A. I was not going to read it all, Your Honor.

Now, in the tests at the Smith Building the standard coil was used, that is a selective receiver was used, and hence the selective method of testing was employed. That is the arrangement of Figure 3, although we could have gotten very much more noise if we used some other method. Mr. Thompson, in his tests——

Q. That is, in the Smith Building tests? A. In the Smith Building—we compared the operation of the receiver as the defendant built it on various wave-lengths after the method shown in Figure 3 of the sketch which
11340 I have just made, and showed that it was tuned, built tuned, and could not be improved by additional tuning when you were in the range of commercial wave-length, but it got progressively poorer at the high wave-lengths, atrociously bad, unless added tuning means were used.

Now, Mr. Thompson in his tests at the Washington University substituted a coil and put it in its tightest, i. e., closest, possible relation to the primary; thereby he destroyed all pretention to selectivity, it was no longer a selective receiver, hence, the means of tuning which he chose to compare it with was that which gives no

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loudness, but selectivity. When I, as an operator, receive, I first tune by the method of Fig. 2. Then, having gotten my station, I immediately proceed to tune by the method of Fig. 3, because I have my station, and I do not want loudness, but I want selectivity. I want to be able to cut out the other fellow. Mr. Thompson, taking a receiver and using it in a condition where selectivity was utterly impossible, applied the selective method of tuning, but he did not test for selectivity, he tested for loudness. He said he proved an untuned circuit was better than a tuned circuit. He did not compare like
things, and did not prove anything. It was only neces- 11342
sary to tune the circuit on the basis on which Mr. Thompson made the comparison to show the complete fallacy of what he said, so Mr. Weagant took the same identical apparatus, tuned it according it to the method of Fig. 4, and showed that if loudness was the criterion then the tuned circuit was very much louder.

Q. What is the diagram you have just described? A. That is F. N. W.-30.

Mr Hughes: We offer it in evidence as plaintiff's exhibit No. 73.

(Diagram F. N. W.-30 received in evidence and
marked "Plaintiff's Exhibit No. 73.") 11343

Q. Mr. Waterman, have you fully explained the tests conducted last Saturday at the University of Washington, and the conclusions Mr. Thompson attempted to draw therefrom; if not, explain briefly what further you have to add? A. I think I have covered the more important parts of them. The important feature is: Mr. Thompson tuned for one purpose and tested for another, hence, his tests proved nothing. The tests conducted by Mr. Weagant took Mr. Thompson's own basis of comparison, namely, loudness of signal, and showed

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that even under the conditions which he chose, conditions where tuning, as he showed, was of no real consequence, even under those conditions the tuned circuit was enormously better than the untuned circuit, upon Mr. Thompson's own criterion of what is good. If Mr. Thompson contemplated using that arrangement, Mr. Weagant showed by a second set of tests that he would do better not to have any secondary coil at all, because he could get as good or in some cases better result by simply connecting the detector across the primary coil and save the expense and added complication of a second
11345 coil.

Q. Mr. Waterman, as I understand the report of the Bureau of Standards and the testimony of Mr. Thompson, Dr. Zenneck in his testimony at page 3347 of the record, he appears to take issue with the conclusion that the ratio of the capacity over inductance of the primary circuit has to do with the number of oscillations; in other words, I understand Dr. Zenneck to suggest that the ratio of capacity over inductance does not have to do with the number of oscillations; what have you to say on that? A. May I have the Zenneck book—Dr. Kolster, Mr. Simpson, Mr. Thompson, myself, and I think Mr. Pickard, have all said that the ratio of capacity to
11346 inductance was an important matter in determining the number of oscillations that take place in a circuit, and that since the Simpson mercury valve transmitter has the large ratio of capacity to inductance it cannot have a large number of oscillations, and I have noted that it is in that respect exactly like the specific transmitter described in the Marconi patent. Dr. Zenneck, I think, has given quite an erroneous impression. I do not think he meant to, it is perhaps due to his lack of command of Englsh. He first criticized my statement on the ground that I had referred to his formula of page 13 of his

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book, where the heading was, "Condenser circuit without spark gap," and that, therefore, I had no right to resort to the formula, but he overlooked the fact that on page 15 he gives the same formula as applying to a spark circuit—I would like to read what he says, if I may have the book. The fact, very briefly, is this: if we have a circuit having a certain condenser and certain inductance and certan resistance, the number of oscillations that can occur in that circuit is absolutely determined by those quantities. If we vary the resistance we will vary the number of oscillations, just as we vary it if
we vary any other quantities. That formula, therefore, 11348
sets a limit to the number of oscillations. That number cannot be exceeded. If the primary circuit is of large ratio of C over L, its maximum number of oscillations is limited by that ratio. What Dr. Zenneck said was that within a quite wide range the spark gap circuit could not have a large number of oscillations anyway, and in that I entirely agree, and it is exactly what I have said. I never knew a transmitter that had a large number of oscillations in its trigger circuit, whether its ratio was exceedingly large or not. He said that over a very wide range the number of oscillations was independent of the ratio of C over L; that is to say, that there was always
a small number. I think that is all he meant. He meant 11349
that the resistance factor is made up of three parts, the fixed resistance, the variable resistance in the spark gap, the resistance variable with the coupling, but otherwise constant, which corresponds to the transfer of energy from the primary to the secondary circuit. Now, those quantities are not independent of one another. The transfer of energy and the spark gap resistance may be related to one another; also the spark gap resistance has a certain relation to the capacity. Dr. Zenneck, therefore, meant to say simply that within quite a wide range

the number of oscillations in the spark gap circuit was so small anyway that the ratio of C over L did not come in as a determining factor, but he did not mean to say that the ratio of C over L did not fix the maximum that could not be exceeded.

Q. He has referred to page 91 of his book, as applying to the Simpson mercury valve transmitter——

August 1, 1916, 9:30 o'clock a, m. Continuation
11351 of proceedings pursuant to adjournment,
all parties present as at former hearing.

F. N. Waterman, same witness, resumes the stand for further direct examination.

Q. (Mr. Hughes) Mr. Waterman, Dr. Zenneck referred to page 91 of his book, seeming to apply it to the Simpson mercury valve transmitter; what have you to say as to the making of such application, and what is the reply thereto? A. The portion of Dr. Zenneck's book to which he referred has to do with the production of coupling waves or beats in two associated circuits of a
11352 transmitter. By applying these statements and mathematical considerations to the Simpson mercury valve transmitter, Dr. Zenneck recognizes the transmitter of Simpson as such a coupled tuned circuit transmitter, as it has been called in this case.

Q. On pages 3344-5 (Print p. 3582) of the record Dr. Zenneck referred to and quoted the following from the report of the committee on standardization of the Institute of Radio Engineers, as a definition of impulse excitation, namely:

"A method of producing free alternating cur-

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> rents in an excited circuit, in which the duration of the exciting current is short compared with the duration of the excited current."

State whether or not this definition necessarily involves two circuits? A. It does.

Q. Does it define any particular structure of those circuits? A. A. It does not in detail define the circuits, but it does require that there shall be an exciting circuit and an excited circuit, and that these shall be associated so that one transfers energy to the other. Dr. Zenneck applies this definition to the Simpson mercury valve 11354
transmitter and thereby recognizes that there is an exciting circuit, namely the closed spark condenser circuit, and an excited circuit, namely the antenna radiating circuit, and that energy is transferred from one to the other by a few oscillations in the primary circuit and the production thereby of free oscillations in the secondary or antenna circuit.

Q. Now, will you state how this definition applies, if at all, to the operation of the apparatus specifically described in the Marconi patent in suit? A. It applies to the operation of the apparatus specifically described in the Marconi patent, in precisely the same way as it applies to the Simpson mercury value transmitter. Mr. 11355
Weagant and I built and tested out four of those specifically described constructions. Three out of the four, without any manipulation or adjustment other than the simple building in accordance with the dimensions given in the patent, operated in this way. There were very few oscillations in the primary circuit, and there were many oscillations in the secondary circuit, and those oscillations in the secondary circuit had the natural period, that is, were the free oscillations of the antenna circuit.

Q. Referring now to the criticisms offered by several
of the witnesses for the defendant in respect to the note
given by the Simpson mercury valve transmitter dur-
ing the tests of Dr. Chaffee at the Cruft Laboratory;
what have you to say on that subject? A. I want first
to point out that the defendant's observers had no
proper opportunity for judging the note in the critical
way in which they have judged it. The wave meter was
altogether too near to the transmitter for that purpose.
I set up the wave meter at the request of the defendant's
observers, and I did not know what they wanted it for.
11357 I set it up in a position suitable, or approximately suit-
able for resonance curve measurement.

In another building on the campus——

Q. (Interrupting) In that connection, before you proceed—you say you set it up at their request? A. I set it up at the request of one of the observers, I think Mr. Simon, and I had no idea what they wanted it for.

Q. Was there any suggestion made as to the manner in which it should be set up, or was there any criticism or objection to it? A. None whatever.

Q. Now, proceed. A. If I had known that they
wanted to judge the note, I would have given them the
11358 opportunity of listening on the apparatus which was
set up in the adjoining building, which we had used to
determine the quality of the note and they might there
have had an opportunity to really know what the quality
of the note was. But they have undertaken to critically
judge of the note from observations made in a location
from which such critical judgment could not fairly or
properly be made. It is customary to allow always for
the unavoidable effects of too close proximity to the
transmitter. In making such allowance the judgment
must be very different from that which they have ex-
pressed.

Secondly, I want to say that the qualities of note that were observed were, as most of the defendant's witnesses state, merely those of multiple discharges. Now, the gap in the transmitter may quench perfectly, as the expression has been introduced in this case; that is, it may operate perfectly, and yet the note be very bad, because the note depends, not on the quenching of the gap so much as on the regularity of the discharge. In the Kolster Bureau of Standards report there is shown an oscillograph showing what I mean by irregularity of discharge. That diagram shows conclusively that the operation of the set was with a very bad, irregular note 11360
as Mr. Kolster operated it in the Bureau of Standards. I refer to the curve, which in my copy (Defendant's Ex. No. 10) is marked "Kolster's Exhibit C," and is called "'Rectified voltage and current." If the court cares to examine it; on the lower line of the oscillograph you will see that there are no successive two of the peaks which are of the same height, and that they occur with more or less irregular spacing. This is the cause of the sort of irregularity of note that was observed at the Cruft Laboratory, and it has nothing whatever to do with, and does not in the slightest way affect the operation of the primary circuit; but it 11361
does affect the ability to get clear photographs, or to see clear images, for the reason that it takes several hundred images to affect the eye; it takes many thousands to affect the plate. If the sparks do not occur at the same voltage, then the straight line diagram taken with the single current deflection must of necessity merely show two lines. That is, the line extending in each direction from the center, but it cannot show the beads or spots of light, because the beads will not be twice in the same place, and, hence, in thousands of

images they will simply give the effect of a line, and thereby showing the presence of oscilliations, but not the exact number of oscillations.

Similarly with the photographs, where the time axis is added and the wave is drawn out into the form of a damped train of oscillations; there, both the maximum—the regularity of maximum and the regularity of the spacing count. And the result, with absolutely perfect operation of the apparatus but with irregularity of the interval of sparking, will be simply a blur. Hence, it is necessary to have great regularity for the purpose of
11363 taking photographs, but it has nothing whatever to do with the quality of operation of the set and the number of oscillations in the primary circuit. This is the respect, in other words, in which the note is important. It is not important at all as determining the quality of operation of the gap.

Q. Was this characteristic of the note observable on July 1st and 2nd when you were present and saw tests that were conducted at that laboratory by Dr. Chaffee? A. The note was good on July 1st and 2nd.

Q. When was this faulty condition of the note principally observable? A. It became very bad early in the morning of July 3rd.

11364 Q. And how was it in the afternoon of July 4th? A. On the afternoon of July 4th the note was good.

Q. You say the note was good on the afternoon of July 4th? A. It was good on the afternoon of July 4th.

Q. Now, Dr. Zenneck has said that the exact number of oscillations in a circuit having a spark gap can not be determined either by the logarithmic or the Taege linear method. Do you agree with that statement? A. Yes, I do. As I have explained in my former deposition, those two methods can only be used to approximately determined limits, and the only inference which should

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be drawn is that the number of oscillations is greater than that indicated by the Taege method, and probably less than that indicated by the logarithmic method.

Q. In his testimony Dr. Zenneck, at least expressed surprise if not criticism that Dr. Chaffee used a dummy antenna in his experiments. What have you to say in respect thereto? A. Dr. Zenneck implied that inasmuch as there were two antennae connected to the laboratory building we should have used the actual antennae. I made inquiries regarding these antennae and I found that they were both much larger than a ship's antenna, and too large for the purpose. Second, if for tests of 11366
this kind we had used the actual antenna the laboratory would at once have been called up by the Navy Department or the radio inspector and told to quit, and failure to quit would have meant loss of license to the institution. Dr. Zenneck, apparently, is not familiar with the laws in this country.

The third reason is that the dummy antenna was what was used by Mr. Simpson at the University of Washington tests, and we did not desire to imply, by a change in that respect that we were criticising that use. Rather, we wanted to exactly duplicate Mr. Simpson s conditions.

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Furthermore, it is a perfectly standard and regular thing, and was used by the Bureau of Standards in the Bureau of Standards tests—that is, the dummy antenna.

Q. He has also intimated a criticism that the instrument used for reading the antenna current was not calibrated; what have you to say on that question? A. We offered on the record, in the testimony at Boston, to calibrate that instrument in the presence of the defendant's assembled witnesses, and they declined the invitation, and I have further to say that a Weston instrument, the highest class of instrument for the measuring of antenna

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current, was provided and the current readings had been observed by Dr. Chaffee and Mr. Weagant and myself and I think also by Professor Morecroft and Dr. Coffin, but, either on the afternoon of July 2nd or the morning of July 3rd, somebody accidentally moved a piece of the apparatus by leaning over the table, and so caused a spark which burned out that instrument, and the instrument which was used in the further tests was one which was borrowed from Dr. Chaffee's laboratory.

Q. He has also suggested the fact that certain other instruments were not present which could or might have
11369 been used for the purpose of making a more critically accurate demonstration. What have you to say on those criticisms? A. I was about to add to my last answer, that I think the criticism of lack of aerial ammeter does not come well, in view of the fact that no such ammeter is furnished with the instrument. There is a place for it on the panel.

Q. Point out the place? A. (Pointing)—but it is not there. All first class apparatus has an aerial ammeter with it.

Q. The transverse brass plate across the center of the marble panel, is what you refer to as the place for the use of the aerial ammeter? A. Yes, sir. If the use of an
11370 ammeter was essential they should have furnished it.

Q. Now, answer my question as to Dr. Zenneck's implied criticism because of the absence of certain other instruments? A. Dr. Chaffee had present an alternating current ammeter in the primary circuit of the power transformer which the defendants could observe if they cared to. There were other instruments available there at hand which they could have had if they wanted them, but no such instruments were furnished with the set.

Q. Were they necessary for the making of the tests that were made, for the obtaining of the results which were obtained by those tests? A. Not in the slightest degree. They had nothing whatever to do with the matter, and so long as we were operating the transmitter at the proper output as determined by the aerial ammeter, and which we had determined by the standard instrument to be always greater than that which Mr. Kolster used at the Bureau of Standards, we had satisfied every requirement of precision in that respect.

Q. Now, Dr. Zenneck refers to the fact that there was occasionally a brushing of the condenser, and has suggested that there should have been a resonance curve taken for the purpose of determining whether the brush- 11372
ing of the condensers had any effect. What have you to say on that subject and what are the facts? A. The brushing of the condenser was not such as to lead one to expect any source of inaccuracy from it. However, had it not been for the fact that we lost a day and a half out of the two days allowed for the tests, by the short-circuiting of the gaps and their repair, Dr. Chaffee intended to take the resonance curve in the presence of the defendant's observers. He only had time, and it was very late in the day of July the 4th, to explain to them what arrangements he had actually used in taking his resonance curve. I was interested in the resonance curve, and I have the data which Dr. Chaffee took. I 11373
saw the arrangement of apparatus that he used for the purpose, but I did not see him actually make the measurement.

Q. Have you drawn the curve from that data? A. Yes, I have.

Q. Will you produce it? A. (Producing paper) It shows by the perfect symmetry of the curve, it is a very beautiful resonance curve, that the brushing of the condensers had no effect whatever.

11374 F. N. Waterman—Recalled—Direct.

Mr. Hughes: The plaintiff offers this in evidence as plaintiff's exhibit No. 74 (reproduced opposite).

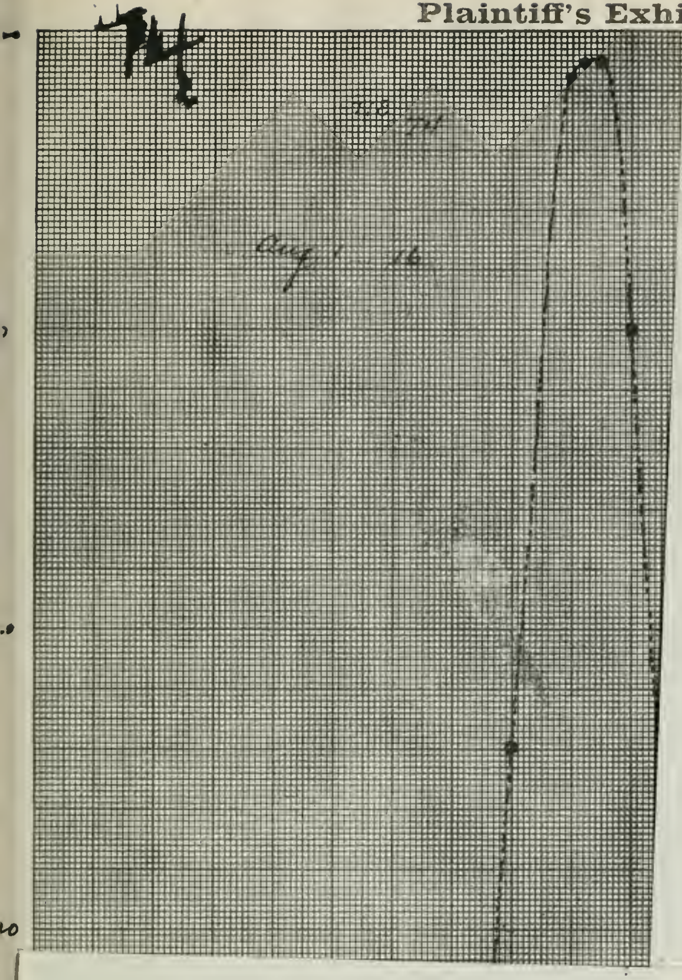
(Received in evidence and marked "Plaintiff's Exhibit No. 74.")

Q. (Mr. Hughes) Dr. Zenneck has also criticised, or made remarks in his testimony that appear to imply at least a criticism of the manner in which Dr. Chaffee associated the deflecting coils with the Braun tube. What have you to say in respect to those criticisms, and
11375 what are the facts? A. Simply this, that in my presence, shortly after the apparatus was set up in the Cruft laboratory, Dr. Chaffee carefully calculated—made elaborate calculations as to what characteristics that deflecting coil circuit might safely have; and having done this he then went through a progressive series of tests to determine whether the proportions used in any way affected the results.

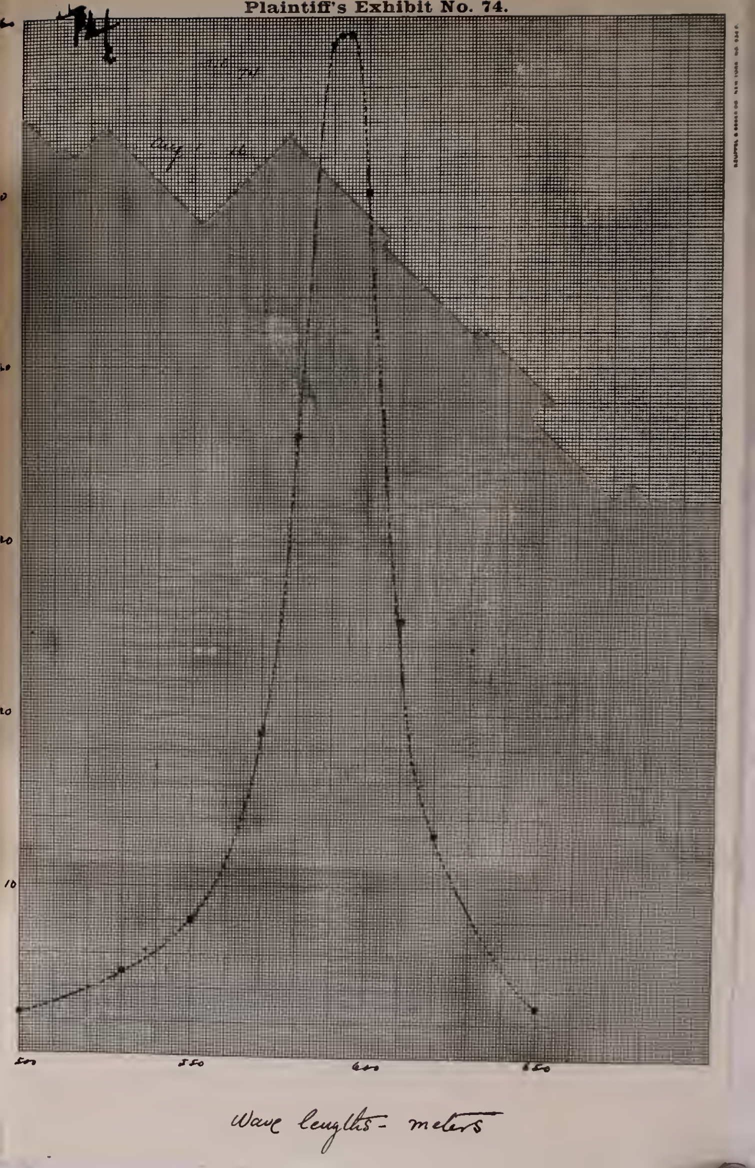
Dr. Zenneck has also in this connection criticised the fact that the leads were too close together; but Dr. Chaffee, in my presence and while I was watching the screen of the Braun tube, varied the separation of those leads from several feet down to as close together as they
11376 could be gotten, and the image on the screen was precisely the same in all cases. Dr. Chaffee took the most elaborate precautions to see that the mode of associating the deflecting coils of the Braun tube with the Braun tube itself and with the transmitter, were not open to any proper or well founded objections.

I am not, personally, an expert in the use of the Braun tube, but I was present when Dr. Chaffee took all these precautions and, while I have no opinion myself, because I am not skilled in the use of the Braun tube, I was very much impressed with the great caution which

Plaintiff's Exhi



Plaintiff's Exhibit No. 74.



Dr. Chaffee displayed, that there should be no proper ground for criticism.

Q. Dr. Zenneck and other witnesses, I believe, commented upon the fact that Dr. Chaffee appeared to choose the times of taking his photographs. What have you to say as to that? A. That criticism is most unjust. I have explained that regularity of operation, in order that several thousand images may be exactly superposed, is quite necessary. It is not easy with such a transmitter to get the extraordinary regularity of discharge—to me it is an incomprehensible degree of regularity—which would permit of the superposing of many thousands of images 11384
with such precision as to take a photograph. What Dr. Chaffee did was simply to have the adjustment of the transmitter for regularity of operation continued until such a state of regularity existed as would permit the eye to see and the plate to photograph what was happening. The same thing was happening at other times; because irregularities of operation have nothing whatever to do with the number of oscillations in the primary circuit. It was, of course, essential to wait until a suitable degree of regularity occurred to permit photographing, before actually exposing the plate.

Q. Now, in the way in which Dr. Chaffee arranged the inside plates in the Braun tube—while it has not been 11385
charged directly that there were any eddy currents, it has been suggested that there might have been; and that has been offered as a further criticism by Dr. Zenneck and other witnesses. What have you to say on that subject? A. Dr. Chaffee, in my presence, carefully checked that matter. He did it by placing the coils at right angles to those plates, so that under no circumstances could there be any eddy currents; and then gradually moved them back to the position where they must be for the purpose of his tests. And he found that the opera-

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tion was exactly the same. The reason they were placed in the position where they were, was that for the double deflection tests; that is, for the tests wherein a time axis was used, and hence the damped train diagram produced, the coil had to be in that position. To find out whether that position introduced any error, Dr. Chaffee carefully tested the apparatus with a straight line deflection and the coils at right angles to the position, and where no eddy currents could be produced, and the results in the straight line deflection diagram were absolutely the same, and the same number of oscillations
11387 shown.

Q. Dr. Zenneck suggested that if the coils had been placed above those plates that possibly the eddy currents could have been avoided; what have you to say as to that? A. Very true; but they could not be placed above, because the space above was occupied by the focussing coil and its supports. They were placed in the only position that they could be placed.

Q. Dr. Zenneck stated that the condenser of the spark gap circuit, is in the antenna, and has also made statements that imply, or may imply at least, that the antenna is directly charged. What have you to say upon that question? A. That this location of the primary or
11388 spark gap circuit condenser has no operative significance is conclusively proven by the fact that when it is taken out of that position the operation of the apparatus is not affected. The tests on this point which were made at the Cruft laboratory were simply equivalent to moving the ground lead from the point s2 on exhibit 68 up to the top of the condenser c. In this case the antenna would be traced from the capacity area f through A g dd[1] and thence directly to earth; and the local, or primary circuit, would be traced from the spark gap through the condenser C, through the common portion of the

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coil dd^1, through the small coil 2a back to the gap. In that case the condenser C is not in anything except the primary circuit, and the apparatus operates in precisely the same way. Thus showing very conclusively that the condenser is not, in any effective sense, in the antenna, and that its presence physically in the ground lead wire has no operative effect.

Further, the tests of Dr. Kolster, the tests at the Cruft laboratory and the tests which had been made in the Marconi laboratory, have shown—and indeed I do not understand that there is any denial of the fact, that
these circuits, the primary and secondary circuit of the 11390
Simpson mercury valve transmitter are, and must be tuned to come to resonance.

Dr. Chaffee has testified that the tuning is very critical; which means that a very small departure from resonance produced a very large effect on the output. This shows conclusively that the energy is in the primary or exciting circuit, and that it is transferred to the antenna circuit.

Furthermore, Dr. Zanneck expressly states this and also, as I understand him, states that it was proven by the Cruft laboratory photographs.

Q. You refer to pages 3342-3 (Print p. 3580) of the
record? A. Yes, I refer to pages 3342-3 of the record. 11391
I will not read it all; although I call attention to the part beginning near the bottom of page 3342 and what I call attention to is the conclusion given by him:

> "I conclude that as long as there was current in the spark gap circuit there was an increase in the amplitude of the current in the dummy antenna".

What that means is——

Q. Prior to that conclusion, he has stated the fact

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that the results obtained by Dr. Chaffee in the circuit, shows that there were two and one-half oscillations, did he not? A. He has. I will continue my answer. What that means is this: While, physically, this condenser C is in the ground lead of the antenna, nevertheless all but about one per cent of the total energy is stored in that condenser; about one per cent of the total energy must be right in the antenna, but that is before the operation begins. As soon as operation begins the spark gap breaks down, the entire energy is then resident in the closed circuit, the red circuit on exhibit No. 68. This
11393 entire energy when the condenser charge has fallen to zero exists as current in the red circuit and not elsewhere; the oscillations of that energy in the closed circuit, as Dr. Zenneck shows in the passage which I have just referred to, builds up, starting from zero, oscillations in the antenna circuit. And I call attention to Dr. Zenneck's blue print curves; that in those blue print curves Dr. Zenneck shows the energy in the antenna beginning at zero, and hence Dr. Zenneck's own diagrams conclusively show that there is no such charging of the antenna in any operative sense, as has been alleged.

Q. Possibly you have explained it clearly enough, Mr. Waterman, and, if so, you need not answer this ques-
11394 tion; but Dr. Zenneck made several diagrams in which he showed the possibility of different circuits; if I may use the term, by manipulating the diagram, and it occurred to me that, possibly, the court might draw the inference from his testimony that the condensers were, or might be by an arrangement of the circuits at a certain time, all in the antenna circuit, and that all the energy stored in them might, by the quenching of the spark gap be left free to radiate in the antenna circuit. Is that deduction one permissible from the facts disclosed here? A. Starting with the progress of events as

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shown in Dr. Zenneck's own blue print of diagrams of oscillations, it is; because those diagrams show that the energy all starts in the closed circuit—none of it in the antenna circuit—and that by the oscillation of this primary or red circuit in exhibit No. 68, it is transferred to the antenna circuit progressively. In the passage which I read, Dr. Zenneck says the energy in this antenna circuit continually increases as long as there is any energy in the primary circuit. Now, when the energy has all been transferred to the antenna from the primary circuit, the spark gap ceases to operate—there is no current there—
and the energy is then in the antenna circuit and is pro- 11396
gressively radiated. That, as I explained in my former answer, is a common characteristic of the specific apparatus described in the Marconi patent and the Simpson mercury valve transmitter.

Q. When the gap sparks, all the energy stored in the condenser leaves the condenser; that is, immediately oscillates or travels, does it ? A. It does. The energy when in the condenser before operation has begun, is merely in a static form. It is like a weight on the roof of a house. As soon as the spark gap discharges then it is like the weight falling off the roof. The energy rushes into and through this primary circuit and it is confined
to that primary circuit until, by the process of oscilla- 11397
tion, and hence transferring through the coupling between the two ciruts it has been built up into quite a different form in the antenna circuit.

For example, that energy, when it is in the red circuit, of exhibit No. 68, that is the primary circuit, is at a very low voltage,—I think it is, perhaps, three or four thousand volts—and a very large current—I do not know just how large it is; but it is of the order of one hundred or two hundred amperes. The current, as it is transferred to the antenna circuit, however, takes a very different form. It is then a current of, perhaps, seven or eight or nine amperes, and fifty or one hundred thousand volts. Showing the complete alteration of the form of the energy. That energy is put into the condenser and then, like a weight falling off the roof; it rushes through the spark gap and becomes resident solely in the red circuit and by its oscillation in this way is transferred to the antenna circuit in a wholly different form, as the result of the coupling and the association of the two circuits.

Q. And then, by the quenching of the spark gap it is
11399 left in the antenna, where it radiates? A. Precisely.

Q. When you were on the witness stand last week you were requested by Mr. Farnsworth to compile a reference to the defenses attacking the validity of the Lodge and Marconi patents in suit, as set forth in Pickard's deposition in this case and you were requested by the court to compare them with the references and defenses in the National suit. Have you made a written statement of those matters in compliance with the question and those requests? A. Yes. I have spent a good many hours in complying with Mr. Farnsworth's request, and I have had my notes written up, amplified a
11400 little.

Mr. Hughes: To save time I may ask that this written report be offered in the record and it will save the time of reading it. That refers to the answer in that case which is necessary to show, the answer setting out the facts, and I offer this as plaintiff's exhibit No. 75, being the witness' response to Mr. Farnsworth's request.

Mr. Skeel: I haven't had time to examine this list prepared by Mr. Waterman.

(List received in evidence marked "Plaintiff's Exhibit No. 75," and copied into the record as follows:)

"WATERMAN'S COMPARISON OF REFERENCES AND DEFENSES AS TO VALIDITY.

NATIONAL ELECTRIC SIGNALLING COMPANY
AND
KILBOURNE & CLARK CASES.

(As called for on pages 3088-3090 in the record.)

In response to the suggestion of the court I 11402
have examined the records in this case and in the National case, and have compared the prior patents and publications referred to by Mr. Pickard in this case with those in the National case.

The following is a list which I believe is complete of the patents and publications referred to by Mr. Pickard in this case and not stricken out by the court. No patents or publications are listed to which he did not refer.

On this list I have indicated by the letter "N" all those patents and publications which were in evidence in the National case. In doing this I have had in mind the fact that the same article is often found in different publications, and the letter "N"
therefore indicates that the same subject matter 11403
was in evidence in the National case either in the same publication or a corresponding one.

I have indicated by the letters "NA" those publications which were in the answer in the National case, but which up to the present time I have not found in exhibits in the National case. It does not follow that they were not in evidence in that case, because the same subject matter is often found in many different publications, and the mass of material in evidence in the National case was so great that a full comparison of the articles involves much labor, and I have not completely checked them. The number appearing opposite each reference is the page number where Mr. Pickard has referred to that article.

N. 486 Marconi, Institute Electrical Engineers, March-April, 1899 Article.
N. 511} 512} Fleming Alternate Current Transformer 1910.
No. 515} 1608} Jones Translation of Hertz Book on Electric Waves, 1890. Viedmans Annalen, 1887-1890 Says it is identical with 1893 and 1894 Ed.
N. 523 Translations of American Inst. of Elec. Eng., May 17, 1893, "Practical Aspects
11405 of Low Frequency Electrical Resonance." "Pupin."
NA. 526 Domalip & Kolacek Annalin de Physique, Vol. 57, pp. 731-750. (Ex. B. 21.)
N. 530 Lecher Annalin de Physique, 1890 No. 12. p. 850.
NA. 531 London Electrician, April 24, 1891, "Effect of Condenser in Alternating Current Circuit."
532 Journal de Physique, 1891, pp. 549-561. Blondet.
532 London Electrician, Jan. 1892. Trouton Article.
NA. 534 Bulletin of Association of Electrical Engineers of the Electro-Technique Inst.
11406 of Montifiori, Leige, Feb. 1898. Della Riccia Article.
N. 540 Reusta de Artigleirra, 1897, Della Riccia.
N. 543,588 Work of Hertz, by Lodge, London Electrician, June 8 & 15, 1894.
N. 544,813 Marconi British Patent 12039 of 1896.
N. 547 Inventions, etc., of Tesla. Martin Book.
N. 571 Tesla U. S. patent 645,576.
NA. 576 Tesla British Patent 20981 of 1896, Fig. 13.
N. 588 Lodge British Patent 11575 of 1897.
589 Lodge Abridgement of British 11575 of 1897.

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N. 591	Recent Progress in Wireless Tel. Marconi, Feb. 9, 1900. (Royal Inst. Tech.)	
N. 594	Braun British Patent—1862 of 1899.	
NA. 595		
818	Braun British Patent 12,420 of 1899.	
N. 595	Stone Patent 714,756.	
N. 622	Translation of American Inst. of Elec. Engineers Mar. 22, 1899, Pupin. (Nov. Dec.)	
N. 624	Ducretet Notice Generale, Oct. 1898.	
N. 630	Tesla Patent 649,621.	
633	Tesla Patent 685,012.	
N. 634	Marconi Patent 627,650.	
N. 634	Marconi Patent 647,007 } Div. corresponding	11408
N. 634	Marconi Patent 647,008 } to Brit. 12326/98.	
N. 635	Lodge Patent 609,154.	
NA. N. 640	Marconi Patent 676,332. (Corresponding with British patent of Marconi 5387 of 1900) Society of Arts Lecture by Marconi.	
NA. N. 642	Marconi British Patent 5387 of 1900. Society of Arts Lecture by Marconi.	
N. 650	Marconi Society of Arts Lecture of May 17, 1901.	
N. 727	Marconi Reissue 11,913.	
N. 788	Marconi Patent 647,009. Div. Corresp. to British 12326/98.	
NA. 788	Marconi Patent 638,315.	
N. 811	London Electrician of 1888. (Same as Alternate Current Transformer.)	11409
N. 815	Ducretet French Patent 288,067. Apr. 21, 1899.	
N. 816	London Electrician of 1899, Marconi. (Same as Inst. of Elect. Eng. 1899.)	
N. 816	London Electrician of 1900, Marconi. (Same as Royal Inst. of 1900.)	
N. 817	Braun Swiss Patent 18,577 of 1899. (Same as German 111,598.)	
N.	(Same as Braun German Patent No. 111,578 &	
N.	Braun British Patent 1862 of 1899.)	

11410 F. N. Waterman—Recalled—Direct.

NA. 1706 High Frequency Oscillation for electric Therapeutics and other purposes. Electrical Engineer. Nov. 1898. (Tesla.)
NA. 1863 Lodge British Patent 29,069 of 1897.
NA. Lodge British Patent 18,644 of 1897.
NA. 1895 Stone Patent 577,214.) Wire Patents.
1895 Stone Patent 638,152(
1895 Stone Patent 726,368
1896 Hutin Patent Le Blanche Patent 838,545.
1897 Bedell Patent 715,537.
N. 1905 Braun Patent 797,544.
(Equivalent British and German in National case.)

11411 N. 1917 "Possibilities of Wireless Telegraph." Pupin. Report from American Institute of Electrical Engineers, November 22, 1899. Fessenden Lecture.

The patents and publications referred to by Mr. Pickard which are not found at all in the National case are eight in number. None of these eight references relate to new subject matter, but are cumulative matter, divisible as follows:

Telegraph, telephony, or power transmission, by wires	4.
State of the prior art	2,
Tesla reference	1,
Lodge reference	1.

11412 The four patents in the first group all relate to telegraphy or telephony by means of wires. They are, Stone patents 638,152, 726,368; Hutin-Le Blanche Patent 838,545; and Bedell Patent 715,537.

The Tesla Patent is 685,012 and it relates to the artificial cooling of coils.

The reference bearing on Lodge is the so-called Lodge Abridgement of British Patent 11575 of 1897, corresponding to the Lodge Patent in suit.

Dr. Kennelly, defendant's expert in the National case, did not consider all of the patents and publications set up in the answer, and not all were offered in evidence. Patents and publications which have been referred to by Mr. Pickard in this case and which were in the answer in the National case but not in evidence either directly or by equivalent publications, so far as yet ascertained, are nine in number. None relate to new subject matter, but are patents or publications which are cumulative in their effect, relating to defenses common to the two cases. They may be subdivided as follows:

Telegraphy telephony, or power transmission by wires, ... 3,
Prior state of the art, ... 1,
Tesla references, ... 2,
Braun reference, ... 1, 11414
Lodge reference, ... 1,
Della Riccia reference, ... 1.

Under the first heading are: London Electrician, April 24, 1891, dealing with the mathematical explanation of the disturbances occurring in a power transmission cable in London, due to the capacity of the cable; Lodge British Patent 18,644, of 1897, "Telegraphy by means of wires"; Stone Patent 577,214, "Telegraphy by means of wires."

The added prior art reference is Annalen Der Physik, Vol. 57, 1895 (B. 21), theoretical article on resonance.

The two Tesla references are, British Patent
20,981, 1896; and Electrical Engineer, November 11415
17, 1898, article on High Frequency Oscillation for Electric Therapeutics and other purposes. These do not apparently add anything new, as compared with what was in the National case, but are merely cumulative.

The Braun Patent is British Patent 12,420 of 1899.

The Lodge Patent is British Patent 29,069, of 1897.

The Della Riccia publication is, Bulletin of Association of Electrical Engineers of the Electro-Technique Inst. of Montifiori Leige, Feb., 1898.

Mr. Farnsworth's question asked me to consider not merely the patents and publications, but the "defenses." The same defenses as to validity were made in each case, except that in the National case there was a defense that Marconi, while his application was pending in the Patent Office, attempted to enlarge the scope of his invention as disclosed in the original application so that it would cover certain inventions made by Fessenden.

I find as above listed that the several defenses on the question of validity were supported by very much the same material in both cases. There are also in this case the above mentioned references
11417 referred to by Mr. Pickard which were not in evidence in the National case. There is also oral and documentary evidence in this case in support of the so-called Stone defense of prior invention which was not in the National case.

The defense of anticipation by Tesla is supported by very large numbers of publications in both cases, those that are new in this case being the two Tesla patents above noted, namely, 685,-012, British Patent 20981 of 1896, and the publication on High Frequency Oscillation for Electric Therapeutics and other purposes.

The defense of prior invention was based in the National case upon the work of Fessenden, whereas in this case it is based upon the com-
11418 munications of Stone.

The defense based on the Della Riccia publications has one new publication in this case, and the Della Riccia writings were not, so far as I can find, used in the National case in support of the defense of anticipation, but only as to prior art.

In order that the court may determine if any omission has been made in respect to the references and defenses in the answers in the National cases I submit herewith copies of the answers in those cases.

(In the original there is inserted a copy of the answers referred to above.)

CROSS EXAMINATION.

Q. (Mr. Skeel) Mr. Waterman, you said yesterday that the tests at the L. C. Smith Building were for the purpose of demonstrating the comparative selectivity of the receiving instruments with and without condensers, is that the fact? A. No. I certainly did not say that, and I did not say anything which could possibly have conveyed that meaning.

Q. What was your point about the selectivity arrangement of the tests at the L. C. Smith Building? A. You have confused two quite different things. What I 11420
said was that at the L. C. Smith Building we tested the defendant's receiver without alteration or modification, for the purpose of showing whether or not it was tuned by its construction to any particular wave length; and the tests showed that it was tuned to the commercial wave length, and that by varying the adjustment of the crystal, the precise resonance point could be varied from 450 to 650 meters. That was what the assessors' report showed.

What I said about selectivity was that since defendant's receiver is a selective receiver, and Mr. Kolster has testified that it has a high degree of selectivity, we 11421
used, as the instrumentalities for testing, the condenser which is applied where selectivity is to be preserved; and that when Mr. Thompson made his tests at the University of Washington he threw away the standard coil; constructed another coil and coupled it in such a way as to entirely destroy selectivity, and yet the method of tuning that he used was the one applicable only where selectivity is wanted.

Q. Were any reports made by the assessors, either at the Smith Building tests or at the University of Wash-

11422

ington tests as to comparative selectivity of the receiver with and without tuning devices added by the plaintiff? A. I do not remember that there were.

Q. In other words, the statement as to comparative selectivity has not been demonstrated by any tests in this case? A. They were agreed to. Mr. Kolster stated that the receiver was——

> Mr. Skeel: (Interrupting) Wait a moment; I am asking you whether they were demonstrated by any tests. Now, if they were agreed to the
> 11423 record will show it.

A. Well, I know selectivity tests were made, Mr. Skeel, and I cannot say for certain without looking up, whether they were reported by the assessors or not.

Q. Is it a fact that any of these receivers' tests of the plaintiff were for the purpose of showing the comparative, all around, over-all efficiency of the receiving apparatus? A. Yes. That was just exactly what the L. C. Smith Building's test was. It showed that the apparatus was constructed tuned; and I have stated several times that the tests showed that for that tuned range the normal commercial wave length, the all around efficiency
11424 of the receiver is very high indeed, but that for other wave lengths it is very bad indeed. That is just exactly what the tests did show.

Q. Then you mean by "all around, over-all efficiency", merely intensity of secondary, don't you? A. No, not at all.

Q. Isn't that the only thing that the assessors reported at the L. C. Smith tests? A. It is the only thing which you ever measure—the only way of measuring resonance, if any, but it is not the only deduction to be drawn. There is no other way of measuring it.

 11425

Q. In operating this defendant's receiver with the tuning devices added by the plaintiff, is it not a fact that, following the example of Mr. Weagant, an operator when he had the instrument on 3000 meters would have connected on to this receiver, in addition to the coil J-1, the large coil JL and the condenser c2? A. Why, no. Nobody would put that arrangement out. Those are testing devices.

Q. Isn't that what Mr. Weagant did? A. Surely; he used those devices for the purpose of testing. If a receiver were to be built to use that method, why, of course, those devices would not be there. Those are just like 11426
measuring instruments. You could not measure a carpet without a yard stick, or test a receiver without having something to test it with.

Q. In other words, you measured the comparative efficiency of our receiving apparatus by applying especially constructed and designed laboratory receiver? A. We did not. Absolutely not. We measured the special coil which Mr. Thompson produced in the condition of tight coupling in which he used it, by applying to it the method of tuning to resonance which is proper and well known by radio men to be proper under those circumstances; and showed that the results which Mr. Thompson got 11427
by an entirely improper method, proved nothing.

Q. Now, when Mr. Weagant then changed the wave length of the secondary from three thousand meters down, so as to receive a message of the wave length of 600 meters, he was compelled to take off the coil JL and to substitute in its place this coil JL_2, and in addition he was compelled to take off the condenser C2 and substitute another condenser, is that the fact? A. I will have to look it up and see (refers). Well, I will have to look at the assessors' report. My impression is that he did not use either of those coils at 600. My note says that the coil JL_2 and JL were both disconnected.

Q. At 600? A. At 600.

Q. And at 800 coil JL_2 was inserted, was it not? A. Yes.

Q. Now, during all of these tests—— A. (Continuing) I might say that that big coil was simply one that we found in Dr. Magnusson's laboratory there and asked him if we could borrow it. Of course it is not to be understood that it is anything except a measuring device.

Q. Now, referring also to the L. C. Smith Building
11429 tests, in that arrangement when comparing the defendant's standard receiver with this receiver with tuning devices added, you changed from one condenser to another as the wave lengths changed, did you? A. Yes.

Q. And you were comparing then the defendant's standard receiver with the coil always the same and always of the same wave length, with a substituted and changed receiver in which the conditions were constantly being changed? A. I see no objection to your putting it that way, if you want to. You have to compare it with something that has been universally agreed upon. Now, the thing that we did was to show that the defendant's receiver when tuned was always—I mean when adjusted
11430 to resonance with the wave length received, was always improved, except in the range where it is constructed in resonance, and there, any attempt to adjust by outside devices spoiled the result; showing that the receiver as constructed is tuned to a commercial wave length.

Q. Do you think that any, except a very skilled operator could possibly operate this receiver with the tuning devices you have added? A. With these particular things?

Q. Yes. A. Well, I do not know. I certainly should not give them to him to operate with. The equivalent thing however is in this Marconi tuner which is in court, and in evidence; and very ordinary men indeed operate it right along.

Q. Do you think that this receiver with these tuning devices added, would be of any practical use commercially?

A. That is not a receiver with something added. It is a receiver with things used to measure and test it with. You have presistently misrepresented that, Mr. Skeel.

Q. Well, did you or did you not add this coil in measuring this instrument? A. We did not add it—we used it. 11432

Q. I will ask you whether or not this receiver, using this additional coil JL and using this additional condenser cl, and using the additional coil JL2 and the other condensers, is a receiver which would be of any practical use commercially? A. If you mean these specific coils, it is not one that I would put out; but it would be very —wonderfully useful if you did not have anything else. But if you mean the equivalent of those coils properly built into the box, then it would be just as effective as the "Achilles" machine—this being the receiver which the defendant built for the Government, if I remember.

Q. Is it not a fact that no operator could adjust his
receiver with the addition of these additional tuning de- 11433
vices, so as to receive a message within a reasonable
time? A. No, absolutely not.

Q. Will you state, Mr. Waterman—— A. (Continuing) I am afraid my last answer may be misunderstood. Your statement is absolutely not true. An operator could, within a reasonable time, and successfully, use that if he had the ordinary degree of skill, but, as I say, I would not put it out for that purpose, because it can be done so much more easily.

Q. Will you state, Mr. Waterman, whether or not the

tests by the plaintiff at the L. C. Smith Building or at the University of Washington—whether or not at those tests any measurements were made, comparing the intensity of the signals on defendant's standard receiver at the low wave length of from three to six hundred or eight hundred meters, with the intensity of the signals of the same receiver at higher wave lengths, say from 1800 to 3600 meters. A. Naturally no; because there is no such method of testing possible. That is an absolutely impossible method of testing, although it looks, naturally, to one who is not skilled, that it might be
11435 done, but it cannot be done.

Q. Then you are not able to say that this receiver does not receive signals on long wave lengths with equal intensity as it does on low wave lengths? A. I surely am.

Q. Will you please point out the measurement from which you draw that conclusion? A. Yes. I listened to that receiver in the Smith Building, to signals coming in at 3300 meters—and if you did not know they are there you cannot hear them—that is signals coming from the Astoria Station of the Marconi Company, but when we put the condenser across, then they are loud, strong and readable signals. Now, when 600 meter sig-
11436 nals are coming a corresponding distance I was able to get a clear, loud signal without the use of any condenser. That is the only test of this sort that is possible.

Q. When were the measurements taken for the resonance curve which you have introduced in evidence this morning on the Simpson mercury valve transmitter? A. They were taken just before Mr. Weagant and I reached the Cruft Laboratory on July 1st, as I understand, because Dr. Chaffee had his apparatus set up.

Q. And none of those measurements have ever heretofore been given to the defendant? A. No, but the defendant could have had them, and would have had the tests made in its presence if we had not lost a day and a half, due to the failure of the Simpson spark gap; as it was, the test that Dr. Chaffee could do out late in the evening was to explain that he had taken such curves, and he displayed all the apparatus with which he took them to the defendant's witnesses, and told just how he made them. Then, as I understood, it was his intention to testify regarding them but also time did not permit.

Mr. Skeel: That is all.

Mr. Hughes: That is all. 11438

(Witness Excused.)

Mr. Hughes: Plaintiff rests.

Mr. Skeel: The defendant desires simply to have Mr. Kolster's testimony on the plaintiff's last receiver test.

DEFENDANT'S FURTHER SURREBUTTAL EVIDENCE.

11439

Frederick A. Kolster, recalled as a witness on behalf of Defendant in Rebuttal, testified as follows:

Q. (Mr. Skeel) Mr. Kolster, did you attend the plaintiff's receiver tests on July 29th at the University of Washington? A. I did.

Q. Will you please give your interpretation of these tests with reference to their showing the characteristics of defendant's receiver. A. The tests made on July 29 at the University of Washington were those tests in

11440

which there was added to the defendant's receiver tuning devices such as loading coil in the detector circuit, as well as a variable condenser, and thus transforming the receiver into a coupled resonantly tuned receiving device. Consequently, these tests, so far as they have to do with the operation of the defendant's receiver as it stands, show nothing at all, except to substantiate the fact that the defendant's receiver detector circuit is untuned.

It has been stated that some tests made previous to the University of Washington plaintiff tests, I think they
11441 called them L. C. Smith Building tests, proved conclusively that the detector circuit of the defendant's receiver was tuned to wave-lengths in the neighborhood of 600 meters, because signals were heard very loud in the short wave-lengths range and not at all at the long wave-lengths range, and the signals were only heard at the long wave-lengths after a tuning condenser was connected across the detector coil. I cannot see how this can conclusively prove that the detector circuit is tuned. It certainly does not prove this conclusively——

Mr. Hughes: If the court please, this is expert testimony, and it is not testimony which is
11442 in the nature of rebuttal respecting the facts of the last tests. This witness has several times testified as an expert, and it does not seem to me that this is the time for the giving of expert opinion except as to matters of description of what occurred and what those things prove.

The Court: Let the testimony be limited to rebuttal strictly, so nearly as it may be.

Mr. Skeel: It is intended to be so.

Mr. Hughes: But the answers of the witness are not so.

 11443

The Court: Proceed.

A. (Continuing) At the plaintiff's tests, as I said before, at the University of Washington on July 29th, there were added tuning devices, loading coil and variable condenser. These devices are not measuring devices. They are elements of the detector circuit for the purpose of tuning, and they transform that detector circuit immediately into a resonance tuned circuit, enabling all the principles of resonance to be used in that circuit. One of the important effects of adding these loading coils—— 11444

Q. JL? A. JL^1 and JL^2——

A. (Continuing)—is that of loosening the coupling between the antenna circuit and the detector circuit, and the fact that this loose coupling was necessary in order to obtain the proper character of signals indicates to one immediately that the principles of resonance are being involved, whereas the fact that with the receiver used as used by the defendant, the coupling between the primary circuit and the detector circuit can be very much increased is an indication of the fact that the detector circuit in that case is not tuned, and that the principles of resonance are not being made use of in that 11445
detector circuit.

Q. Have you finished all that answer? A. Yes.

Q. You heard the statement of Mr. Weagant that when Mr. Thompson used the closer coupling with the reconstructed coil J1 that he threw away all selectivity of the instrument in order to secure louder signals; please state whether or not you agree with Mr. Weagant, and give your reasons briefly. A. I have not been able to find in the data of any tests made, and particularly in the last tests at the University of Washington, any

11446

evidence whatever that the selectivity had been thrown away. Now, one can be very readily misled by the fact that this coil J1 of the defendant's receiver is very closely coupled by the fact that it is entirely inside of this coil, the primary coil. As a matter of fact, it is not necessarily true that that is very closely coupled, particularly when there is besides the primary coil for the detector coil an additional loading coil in the receiver. The coupling between the primary circuit and the detector circuit is not determined entirely by the fact that the detector actually is entirely enveloped by the lower
11447 part of the primary coil in the defendant's receiver, but it is also determined by the amount of loading coil which is not in proximity or in inductive relation with the detector coil. Now, if it were found that the selectivity had been materially reduced by having this coil J1 entirely inside of the primary coil it would be very easy to restore the selectivity by sliding it back a little ways, and, as I said, I have not found any indication in any of the tests that the selectivity was entirely thrown away when this coupling between the coil J1 and the primary was made as close as is indicated when the detector coil is entirely enveloped by the primary coil.

11448 I would like to add further that apparently there has been some confusion as to whether the receiver was to give the loudest response or maximum selectivity, and I may say that that in the defendant's receiver may be controlled at will by the mere sliding back and forth by the coil J^2.

Q. What does that do? A. That merely changes the coupling between the antenna circuit and the detector circuit. It determines the amount of energy which will be taken away from the antenna by the detector. Now, it is very often desirable to get loud signals at the sacri-

fice of selectivity, and operators in practice, operating receivers, often deliberately make this coupling very close, as close as they can, when they are listening for a signal. In that case their selectivity is perhaps not so good as it might be, but they are able to receive or hear a call which may be coming in readily, and if after they once get their call they desire to restore selectivity, perhaps at the sacrifice of strength of signal, and perhaps not, they can readily do that by loosening the coupling between the antenna circuit and the detector circuit. And in very many cases in practice it is cus-
tomary for the designer of the receiver to perhaps limit 11450
the range of coupling which can be obtained between such two circuits as in the defendant's receiver, limit it so that an operator cannot increase his coupling so much as to entirely destroy his selectivity, or make the selectivity very small, and limit the position so that a sort of a happy medium is obtained where the signal is reasonably strong and the selectivity is reasonably good.

Q. Does the same answer, Mr. Kolster, apply, or did you wish to add anything, in reference to Mr. Waterman's statement that defendant's receiver could have no selectivity when the close coupling was used with
the coil J^1, and that, therefore, the detector coil might 11451
just as well be thrown away and the detector connected to the antenna coil? A. I have not been able to find in the results of any of the tests any indication whatever that when that coil J-2 is entirely enveloped by the primary coil that the selectivity is entirely thrown away, and I think perhaps that Mr. Waterman's statement was based on the last experiment of July 29th, where the detector was removed from the secondary coil and placed directly across the primary coil. I would be very unwilling to conclude from that experiment that the selec-

tivity had been entirely thrown away, because of the result of that experiment. The character of the antenna circuit is immediately changed by placing the detector across the entire inductance of that antenna, and to compare what happens under those conditions with what happens when the detector is removed from the antenna and put across a separate coil requires some further thought and experimentation, and I would not be willing to say that that experiment proves conclusively that the selectivity is thrown away. The trouble with a good many receiving tests is that it is very difficult to make
11453 such tests without perhaps changing two, or perhaps three things at once, and unconsciously changing two or three things at once and then considering only one of those as producing an effect.

Q. Do you consider the tests at the University of Washington on July 29th fair tests for showing the all-around efficiency of the defendant's receiver?

CROSS EXAMINATION.

Q. (Mr. Hughes) Were you present at the tests conducted by Mr. Thompson on July 20th and July 22nd at the University of Washington? A. Yes, I believe I
11454 was.

Q. At that time he disconnected the secondary coil of this receiver entirely, did he not, the secondary coil built in as part of this receiver and used as part of the receiver in commercial operation? A. Yes, that coil was removed and replaced by other coils.

Q. The coils that you have spoken of as J-1 and J-2, I believe, were the coils substituted, were they? A. Yes, they were.

Q. Do you wish to be understood as saying that with coils J-1 and J-2 it is the same receiver as it is when it has its original coil? A. I would be understood that it is the same type of receiver.

Q. Do you want to be understood that the coil built into this receiver and arranged for association with the primary coil, as in the receiver as constructed, is the equivalent of the substituted coils in all respects? A. Well, that would depend on what you mean by the equivalent. It is exactly similar so far as the type of receiver is concerned.

Q. Why didn't Mr. Thompson use the coil as constructed instead of substituting a different coil, differently arranged and differently associated with the pri- 11456
mary coil? A. The essential difference between the receiver with the standard coil and the new coils J-1 and J-2 are practically only mechanical differences.

Q. Then why—— A. The purpose of the new coils J-1 and J-2 being, among other things, to permit of more coupling.

Q. Of closer coupling? A. Of closer coupling between the primary and detector circuit.

Q. They would permit of very much closer coupling, would they not? A. I do not know. I have not measured how much increase there is.

Q. Can you give any other reason for substituting
the coil actually used in the receiver for the coils used 11457
in making the tests by Mr. Thompson on July 20th and July 22nd? A. Yes, particularly the coil J-2. That coil was carefully constructed to have a small distributing capacity as can be obtained readily on that form, thereby making the free period of the coil itself extremely small, very much smaller than the standard coil used. In fact, the winding of coil J-2 has been separated, the turns have been opened out, and the lengths of wire are much shorter than on the standard receiver, and it has

also been mentioned that that coil can also be placed or moved into the primary coil to get closer coupling.

Q. Now, as a matter of fact, J-2 was constructed so as to have about—with inductance enough to produce about a 300 meter wave, and they subsequently took part of the wiring away to bring it down to 200 meters or less, during tests? A. As I understand it, it was originally wound, when it was originally wound it had a natural period with all the detector circuit connected to it, of 280 meters, I think, or thereabouts, and a few turns were deliberately taken off to reduce that to still shorter
11459 wave-length.

Q. You say operators often couple tight; isn't that purely for the purpose of hearing anything that comes? A. Yes, for listening in.

Q. But if they want to select from among the signals that are coming, excluding certain ones and receiving others, then they must couple more loosely? A. Yes.

Q. And the loose coupling is necessary for the purpose of selectivity? A. Yes.

Q. How much time have you been present in the trial of this case?

Mr. Skeel: I object to that as incompetent and
11460 immaterial, and the record will show.

Mr. Hughes: No, the record does not show.

The Court: Objection sustained as to the time he was here.

Mr. Hughes: Well, I want to show the interest of the witness.

Q. Are you still in the Government service? A. Yes, sir.

Q. You were sent here by the Government to testify on this last occasion? A. Yes sir.

Q, Are you here in the pay of Kilbourne & Clark? A. No sir.

Q. When did you leave Washington to be here at this hearing? A. July 17th.

Q. Is it part of your official duty to be here as an expert witness in the trial of this case, or do you volunteer that? A. I was authorized to come here by the Government.

Q. Were you ordered or directed to come here? A. I was authorized to come here.

Q. Well, I have put another question; were you ordered or directed to come here? A. Practically so, yes 11462
sir.

Q. You were the principal spokesman for the defendant at the last tests at the University of Washington, making objections and pointing out matters that you demanded the Assessors should note on behalf of the defendant, were you not? A. I was not the principal spokesman, but I did make one or two comments.

Q. You on at least two or three different occasions demanded that the Assessors should note certain facts, did you not? A. No sir, I did not demand anything. I simply asked if the Assessors would make that note.

Q. And you did that oftener than anyone on behalf
of the defendant, not excepting Mr. Thompson himself, 11463
did you not? A. I think I made two requests.

The Court: Anything further?
Mr. Hughes: That is all.
Mr. Skeel: That is all.

(Witness Excused)

Mr. Skeel: The defendant rests.
The Court: Have both sides concluded?
Mr. Betts: Yes, Your Honor.

11464 (Discussion as to division of time for argument)

Mr. Skeel: I wish to ask one more question, if the court please; Mr. Cosgrove yesterday, I think, asked if I was going to have up one of the impulsive transmitters to use to refer to in the argument. Now, we have just one at the shop that is supposed to go out to-morrow. If plaintiff desires it we can have it here, I think, for a day or so. Do you desire that, Mr. Betts?

Mr. Betts: I think I would like to have it here, because the Thompson transmitter was offered in evidence with the idea that you could take it away
11465 and produce one at the trial in this court and the Circuit Court of Appeals.

Mr. Skeel: We agreed to produce one for argument when requested, and if counsel desires it we will produce it here at 1:30 this afternoon.

The Court: Something was said sometime ago, I think, about presenting briefs. Was it the purpose to supplement this oral argument by written briefs?

Mr. Hughes: Yes.

I think that was understood by court and counsel on both sides.

11466 Mr. Betts: Yes.

OPINION FILED DECEMBER 11, 1916 (239 F. R. 328). 11467

NETERER, District Judge:

The plaintiff alleges infringement of two letters patent, #609,154, issued August 16, 1898, to Lodge, and #763,772, issued June 28, 1904, to Marconi, and each held by complainant. Defendant denies infringement, and alleges that the claims made by Lodge and Marconi were patented prior to the Lodge and the later Marconi invention or discovery, by the issuance of patents to Marconi, Pupin, Tesla, Fessenden, and various other
patentees. The record in this case is very voluminous, 11468
and many phases of the electrical art have been exploited. The court room was converted into a laboratory. The electrical apparatus was set up and operated in the laboratory of the defendants. Demonstrations were made at the Seattle office of the complainant, and likewise in the laboratory of the University of Washington. Demonstrations were made of plaintiff's apparatus, and of the Lodge patent, of the Tesla patent, and defendant's apparatus, and messages transmitted to and received from a station at the Navy Yard, Bremerton, Washington. Renowned physicists have testified
for days, and have unfolded to the court the principle 11469
upon which and theory by which the various appliances are operated. Notwithstanding the voluminous record in this case, the issues of fact are few, and aside from the history of the prior art, would make a limited record. (1) It will not be necessary, in view of the very full history of the prior art set forth by Judge Townsend, in Marconi v. De Forest (C. C.), 138 Fed. 657, and Judge Veeder, in Marconi Wireless Telegraph Co. v. National Electric Signal Co. (D. C.), 213 Fed. 815, to review the prior art, except as it may have relation to resonance, and double circuit tuning of the transmitting and receiv-

11470 ing apparatus. Since the commencement of this action, the Lodge patent has expired. This patent has been repeatedly adjudicated, and without further discussion, and without passing upon the merits, I think we may say that further inquiry as to its validity is not demanded. It is conceded that if the Lodge patent is valid, it has been infringed.

(2) Defendant claims to use the principles and fundamental apparatus of the first Marconi, 1896, patent, expired, and Lodge patent, expired, and the high spark frequency suggested by Professor Pupin, in 1899. Marconi uses Hertz oscillations or electric waves, and in the
11471 specifications of his patent #763,772, says he has:

> "invented certain new and useful improvements in apparatus for wireless telegraphy,"

which,

> "relate to apparatus for communicating electrical signals without wires and by means of Hertz oscillations or electric waves.
>
> The object of the invention is to increase the efficiency of the system and to provide new and simple means whereby oscillations or electric waves from a transmitting-station may be local-
> 11472 ized when desired at any one selected receiving-station or stations out of a group of several receiving-stations.
>
> To provide a transmitter which by suitable adjustment will, as the patent states, localize or select any desired receiving-station. In other words, the waves radiated from such a transmitter will affect only a single desired receiving-station out of a number of different and distant receiving-stations.
>
> According to the present invention, the system includes at the transmitting-station the combination, with an oscillation-transformer, of a kind suitable for the transformation of very rap-

 11473

idly alternating currents, of a persistent oscillator, and a good radiator, one coil of said transformer being connected between the aerial wire or plate and the connection thereof to earth, while the other coil of the transformer is connected in circuit with a condenser, a producer of Hertzian oscillations or electric waves shown in the form of a spark-producer, and an induction-coil (constituting the persistent oscillator) controlled by a signaling instrument),''

the term ''persistent oscillator'' being

> ''an electrical circuit of such a character that if electro-motive force is suddenly applied to it and the current then cut off, electrical oscillations are then set up in the circuit which persist or are maintained for a long time,'' 11474

and a good radiator,

> ''An electrical circuit which quickly imparts the energy of electrical oscillations to the surrounding ether in the form of waves, being rapidly radiated in the form of electric waves by the electric conductor, the approximately closed cirsuit of the primary being a good conserver, and the open circuit of the secondary being a good radiator of wave energy.'' 11475

The patent also provides:

> ''At the receiving-stations employing my present invention, I prefer to use a receiver such as those described in my several U. S. patents, Nos. 586,193, 627,650, 647,007, 647,008, 647,009, 668,315, capable of being affected by electrical waves or oscillations of high frequency.''

Below is shown diagrammatically the Marconi patent applied for in 1896, and reissued as #11,913, shown at

11476

the left, and the Marconi patent in suit, shown at the right (See Transcript, Vol. 1, p. 314, for drawing.)

The transmitter of the patent in suit consists of an association of two circuits—a closed circuit, *G d, e,* and an open radiating or antenna circuit, *F A D1 E*—Of this association, the specifications, lines 33 to 36, page 2, say:

> "Alternating currents of high frequency pass through the primary of the transformer (C) and induce similar oscillations in the secondary,"

11477 and page 2, lines 12 to 20:

> "My experiments have demonstrated that the best results are obtained at the transmitting-station when I use a persistent oscillator—an electrical circuit of such a character that if electromotive force is suddenly applied to it and the current then cut off, electrical oscillations are set up in the circuit which persist or are maintained for a long time—in the primary circuit."

The primary circuit, therefore, is a persistent oscillating circuit.

The characteristics of the radiating circuit are expressed, page 2, lines 20-24, as

11478

> "a good radiator, i. e.-, an electrical circuit which very quickly imparts the energy of electrical oscillations to the surrounding ether in the form of waves—in the secondary circuit."

The inductive linking of these circuits is spoken of, page 1, lines 56-62:

> "The system also requires as essential elements thereof, the inclusion in the lines (at both stations) from the aerial conductor to the earth, of variable inductances, and the use at both stations of means for varying or adjusting the in-

 11479

> ductance of the two circuits at each station to accord with each other."

An essential element of each of the circuits is variable means of adjustment, which is indicated in the radiating circuit, by coil *1,* and in the closed or persistent oscillating circuit, *G. d. e.,* the object being to adjust the circuits to be in accord with each other. Specification, lines 62-63, page 1:

> "By this arrangement of apparatus I am able to secure a perfect 'tuning' of the apparatus at a transmitting-station." 11480

The definite purpose of the inventor with relation to adjustment of these circuits is more clearly set out, lines 118-129, page 2, in which he says:

> "The capacity and self-induction of the four circuits, i. e., the primary and secondary circuits at the transmitting-station, and the primary and secondary circuits at any one of the receiving-stations, in a communicating system, are each and all to be so independently adjusted as to make the product of the self-induction multiplied by the capacity the same in each case, or multiples of each other,—that is to say, the electrical time periods of the four circuits are to be the same 11481
> or octavos of each other,"

Page 3, lines 5 to 17:

> "If the time periods of the circuits of the transmitting-station are varied until they are in resonance with those of one of the receiving-stations, *that one alone* of all the receiving stations will respond, provided that the distance between the transmitting and receiving stations is not too small."
>
> "The adjustment of the self-induction and capacity of any or all of the four circuits can be

11482 made in any convenient manner and employing various arrangements of apparatus, those shown and described herein being preferred."

It is also said, page *1*, lines 56 to 66:

> "The system also requires as *essential* elements thereof, the inclusion in the lines (at both stations) from the aerial conductor to the earth, of variable inductances, *and the use at both stations of means for varying or adjusting the inductance of the two circuits at each station to accord with each other.* By this arrangement of apparatus, I am able to secure a perfect 'tuning'
> 11483 of the apparatus at a transmitting-station, and at one or more of a number of receiving-stations."

The source of power is connected with the primary circuit, which contains a condenser and the two circuits associated inductively, and the oscillations created in the primary circuit transferred to the antenna circuit.

The specifications further say:

> "In operation the signal key *B* is pressed, and this closes the primary of the induction coil. Current then rushes through the transformer circuit, and the condenser *E* is charged, and subsequently, discharges through this spark-gap. If the capac-
> 11484 ity, the inductance, and the resistance of the circuit are of suitable values, the discharge is oscillatory, with the result that alternating currents of high frequency pass through the primary of the transformer and induce similar oscillations in the secondary, these oscillations being rapidly radiated in the form of electric waves by the elevated conductor."

The 1896 patent provides a transmitting-station, comprising an elevated capacity, *f*, connected by means of the antenna, *a*, and spark-gap, *g*, and thence to earth, and a source of energy, battery *a*, telegraph key, *b*, and

 11485

an induction-coil, *c*. When the circuit of the chart in yellow is closed by pressing down the key *b*, the current will flow from battery *a*, through the circuit, and the primary induction-coil is energized, and that energizes the secondary of the induction-coil, *c*, and the current is thereupon imparted to the spark-gap, *g*, and when the resistance of the spark-gap has been overcome, it breaks down, and the surgings across the spark-gap produce oscillations in the antenna, *a*. The receiver has an aerial or antenna, *a*, connected to capacity, *f*, at the top, and earth, *e*, at the bottom. In the antenna circuit is a detector, *t*, connected to a local battery circuit, *c1, r, b*, and 11486
c2 and *t1*, so that when the energy is radiated from the transmitting antenna and impinged upon the receiving antenna, the detector *t*, is operated, and in turn operates the telephone, which is indicated by *r*. The spark-gap and the detector are in the transmitting and receiving circuits, respectively. The energy from the transmitting antenna is all radiated in two or three oscillations, being one big oscillation followed by two or three small ones. The antenna is a good radiator, and, therefore, not a persistent oscillator. The receiving antenna is a good absorber.

In the patent #763,772, in suit, Marconi removed the spark-gap from the antenna circuit and put it in another 11487
circuit, *G e d* (red), in which circuit he put a condenser, so as to form a closed circuit. This closed circuit does not radiate energy, and is known as a persistent oscillating circuit.

Marconi says:

> "The approximately closed circuit of the primary being a good conserver, and the open circuit of the secondary being a good radiator of wave energy,"

11488 Opinion.

and the purpose of the primary circuit being to slowly furnish the stored energy to replace that radiated through the antenna circuit, to do so, the circuits must be in harmony with each other, tuned together.

To efficiently utilize the energy in the closed or primary circuit, it is necessary to associate it with the radiating circuit through a transformer or other means, and bring them into harmony or tune, and when they are in tune, the energy is transferred from the primary to the antenna circuit, and radiated.

> "Similarly two circuits of the receiver are
11489 linked through a transformer so that electrical oscillations in the open or absorbing primary build up similar oscillations in the closed or conserving secondary, until the coherer breaks down."

and,

> "finally the four circuits must be tuned together."
> Marconi Wireless Telegraph Co. *v.* National Electric Signal Co. (D. C.), 213 Fed. 815."

In the first Marconi patent, it is said:

> "According to this invention, I transmit through the air, earth or water, by means of oscillations of high frequency * * *,

11490

and again,

> "When transmitting signals through the earth, I connect one end of the oscillation producer and one end of the circuit closer to earth and the other ends to plates, preferably electrically tuned with each other in the air and insulated through earth."

In his Nobel prize lecture, Marconi says:

> "A very small amount was used, the high tension current being produced by an ordinary

 11491

Rhumkorf coil. The main feature of my system consisted of elevated capacity areas or antenna attached to one pole of the high frequency oscillators or receivers, the other pole of which was earthed * * *. Many technical writers have stated that the elevated capacity at the top of the vertical wire is unnecessary * * *. The necessity or the utility of the earth connection has sometimes been questioned, but in my opinion, no practical system of wireless telegraphy exists where the instruments are not connected to earth * * *."

"Physicists seemed to consider for a long time that wireless telegraphy was solely dependent 11492
upon the effect of free Hertzian radiation through space, and it was years before the probable effect of the conductivity of the earth between the stations was satisfactorily considered or disclosed."

Again, he says:

"A remarkable fact, not generally known, in regard to transmitters, is that none of the arrangements employing condensers exceed in efficiency the plain elevated aerial or vertical wire discharging through earth through a spark gap as used in my first experiments."

and, 11493

"By means of the sharp gaps in compressed air, and the addition of induction coils placed between the aerial and the earth, the system can be made to radiate through pure and slightly damp waves, eminently suited for sharp tuning."

Many statements are made, confirming the conducting functions of the earth. Professor Pupin, replying to Marconi's New York address, said:

"The first time wireless telegraphy of the present day was produced was when Mr. Mar-

> coni, in 1895, connected his sending wire to the ground and his receiving wire to the ground and let the spark go. That was the first wireless wave of today, and it was not a hertzian wave, and has nothing to do with it. If we are to call it a wave, let us call it a Marconi wave."

He further stated:

> "You have seen that Mr. Marconi uses condensers and spark-gaps and interrupted currents * * * I do not believe in condensers and spark-gaps—if I can get a real alternator, give
> 11495 me a high power alternator that will give 40,000 vibrations per second * * *. I would not express that opinion so boldly, perhaps, if I were not backed up by Mr. Marconi * * * That is what Mr. Marconi wants; and he will have it if I can help him. With 100 kilowats at 25,000 cycles, we should be able to telegraph to Australia. Distance means nothing. Hitch your wagon to the earth and shake it hard enough, and there is no reason why you should not shake a message to Australia."

In the New York Electrical Society address, Marconi said:

> 11496 "The arcs consisting of the condenser circuit and the elevated aerial or radiating circuit are more or less closely connected to each other by adjusting the inductance in the elevated conductor, and by the employment of the right value of capacity or inductance required in the condenser circuit, the two arcs were brought in electrical resonance a condition which I first pointed out as being essential in order to obtain sufficient radiation and good tuning.
>
> * * * * * * * * *
>
> "These two circuits are tuned so as to have approximately the same natural period of electrical oscillations."
>
> * * * * * * * * * *

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"It is well known that when using ordinary spark discharge in the primary circuit, unless weak coupling is employed, the oscillations set up in one circuit create oscillations of two frequencies in both circuits. This has the disadvantage that the radiated energy becomes divided between two waves of different length, and if the receiver is tuned to only one of these wave lengths, it will utilize or absorb only part of the energy reaching the receiver—the energy of the other wave being lost."

* * * * * * * * * *

"As it would have been too expensive to use vertical wires of very great height, the only alter- 11498
native was to increase their size or capacity * * *."

Tesla, in his book, page 213, with relation to resonance, says:

"To produce the best results, it is, of course, necessary to adjust carefully the capacity of the jars, the arc between the knobs and the length of wires. My experience is that calculation of the length of the wires leads, in such case, to no result whatever. The experimenter will do best to make the wires at the start very long, and then adjust by cutting off first long pieces, and smaller ones as he approaches the right length." 11499

The principle of resonance was old in the art, but apparatus by which to accomplish it was in the experimental stage. Resonant tuning implies accumulative action or building up by successive increments or pulses or waves from one circuit into another. "Resonant" is defined by the Standard Dictionary, 1895 Edition.

"Resonance, n. 1. The quality of being resonant; the act of resounding.

11500 2. Physics. (1) A *prolongation* or *reenforcement* of sound by means of sympathetic vibration or the capability of producing such a *continued* sound. (2) By extension, the *increase* of vibration of any kind, as in electricity, by an intermittent force of the *same period.*

"The principle of resonance depends on the fact that to increase any movement of the nature of vibration or oscillation, the force applied must act intermittently in the same period, as when a child moves a heavy swing by pushing always at the moment when the push has the greatest effect. The impulses are thus added together. An ob-
11501 ject capable of vibrating can always be set in motion in this manner by a neighboring object whose vibrations are in the same period, as when a note played on a musical instrument causes the same note on a neighboring stringed instrument to sound. The sounding-board of a piano, guitar, or the like, is capable of responding to vibrations of many periods, and hence reenforces all tones equally; the air in a flue organ-pipe on the other hand, can respond to one note only, and hence selects that one from the fluttering noise produced by the air at the lip. In electricity the principle is valuable in detecting and investigating electro-magnetic waves."

11502 Judge Veeder said: Marconi Wireless Tel. Co. v. Nat'l Electric Signal Co., supra:

"Resonance is an increase or amplification of the periodic motion by an intermittent force of the same frequency. A certain or natural period of vibration is characteristic of all bodies which, when displaced by the application of external force, tend, by virtue of their elasticity, to return and to execute free vibrations until, by virtue of their exertion, they gradually come to rest. Sonorous bodies such as strings under tension and confined portions of the air, as in the organ

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pipe, are further illustrations suggested by the term. Just as very feeble impulses applied to a pendulum at rest, at intervals *exactly* corresponding to its natural period of vibration, will cause almost any desired amplitude of swing, so bodies capable of executing vibrations by use of their own resiliency, may be put into strong vibration by a *series of impulses* in tune with their own natural period. Thus impulses from a tuning fork will cause another tuning fork of the same pitch to hum a note in unison. Resonance effects may likewise be observed in the flow of electricity in a circuit. A circuit possessing inductance and
capacity has a certain time period of vibration 11504
* * * * * Such a circuit is said to have a definite wave length. A circuit possessing capacity and inductance tends to oscillate electrically at its own frequency. It becomes the seat of the *induced* oscillatory current when subjected to the influence of electric waves of that frequency, each wave giving a slight impulse to the oscillations already excited, with the result that the induced electromotive force will be amplified in intensity, just as the swing of a pendulum is increased by the application of properly tuned though feeble touches. However, not only must the impulses, of whatever kind, be rightly timed, *but it is essential* to the utilization of resonance that there
should be a long series of such impulses of ap- 11505
parently equal strength or amplitude. Having regard to ether waves, *such a train can only result where the oscillations from which they proceed occur in the circuit which gives out its energy slowly, for the amplitude of the waves depends upon the energy expended.*"

To reconcile persistency of oscillations and amplitude of vibration in the radiator was a real accomplishment. Induction-coils, generators, primary circuits, spark gaps, condensers, inductors and the various elements

11506 that make up radio-telegraphic circuits are means of getting into the antenna, oscillations both of the persistent nature and at the same time of great strength or amplitude. Marconi associated a circuit, an oscillator, which left to itself, threw off its energy rapidly, and the oscillations in which would be rapidly damped, with another circuit, which was a good conserver of energy, and instead of giving off the energy rapidly in one big wave, withheld it and gave it off slowly and fed it gradually into the antenna as it was radiated out into space, which he called the reservoir circuit. He desired to associate
11507 with his aerial a reservoir circuit as a conserver of energy, so that the antenna would have a persistent train of oscillations. The characteristics of the patent in suit are two circuits, the oscillator or antenna, which is a good radiator, associated with a circuit which is supplied with energy by a source of power, and which is so proportioned as to be a conserver of energy or a reservoir, or a persistent oscillator. Lodge's idea was to throw the energy into the antenna just as quickly as possible, so as to leave the antenna free to vibrate without interference of other circuits.

The Marconi receiver in issue has two circuits of like characteristics, the antenna, being a good absorber, as-
11508 sociated with the closed circuit, which is a good accumulator, this latter circuit to be so proportioned as to be a good accumulator as distinguished from a good absorber, and the energy being transferred by resonance from the primary to the secondary in each case, so as to build up gradually and maintain oscillations, each of the four circuits to be adjusted or tuned to the same time period by adjustable means in each circuit, enabling independent varying of its time period for change of wave length.

Judge Veeder, in Marconi Wireless Telegraph Co. v National Electric Signal Co., supra, said:

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"The essential features of this apparatus and its departure from previous methods of operation are apparent. In his first patent Marconi had disclosed a method and apparatus for the effective transmission of wave energy through the ether of space, and for its utilization in communication of intelligible signals. But in this early apparatus the energy was quickly radiated and as quickly absorbed. By reason of this characteristic his radiator could not create, nor could his receiver store up, the effect of a sustained train of waves necessary for the utilization of the principles of resonance. It was an effective apparatus for distress calls and purposes of that kind, but there 11510
was necessarily interference between messages. Moreover, the electric energy that he could get into his transmitter was necessarily limited. The energy supply had to be adapted to the elevated conductor. The capacity of a vertical wire is not great, and the extent to which it may be increased by lengthening the wire or adding capacity areas is obviously limited. Lodge came forward with a new idea. Although he recognized the impossibility of having a circuit which should be at once a good radiator or absorber and a persistent oscillator, he proposed a compromise. He increased the persistence of vibration of his radiating circuit at the expense of its radiating qualities, and increased the accumulative power of his receiving 11511
circuit at the expense of its absorbing qualities. Effecting this compromise by means of the introduction of an inductance coil in an open circuit, he obtained a train of waves of approximately equal amplitude and thus rendered effective syntony possible. But the syntony thus obtained was utilized for selectivity alone. It was attained at the expense of the radiating and absorbing qualities of the circuit; and Lodge still supposed that for distant signaling the single pulse or whip crack was best.

Marconi's improvement, in his second patent, upon his own prior apparatus, and his solution of

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> the difficulty involved in Lodge's compromise, consists in the substitution for a single circuit in both transmitter and receiver of a pair of circuits, one of which is so constructed as to radiate or absorb readily, and the other to oscillate persistently and be a good conserver of energy. By using two linked circuits in his transmitter, in which the circuit of the primary contains a condenser of any desired capacity, with the usual provision for its discharge through spark gap, and in the circuit of the secondary the vertical wire, any required energy may be imparted to the radiator, since the closed circuit of the primary is a good conserver
> 11513 or reservoir of energy for the radiating open circuit of the secondary. This arrangement would be futile, however, without means whereby the stored energy of the reservoir circuit could be transmitted to the elevated conductor at the rate at which that conductor could effectively radiate it. The mode of getting the energy from the reservoir circuit into the radiating circuit, in like measure as it is radiated, is the tuning of the persistently oscillating circuit to the radiating circuit."

Judge Parker, In Re British Radio T. & T. Co., Ltd., which case is cited by plaintiff, in passing upon the English patent in which the expression "persistent oscilla-
11514 tor" is neither used nor defined in the specification, says:

> "As I interpret the patent, the essential features of the invention thereby disclosed are as follows: In order to get over a well-known difficulty in applying the principle of resonance as between transmitter and receiver in a system of wireless telegraphy, a difficulty involved in the impossibility of a single circuit being at once a good radiator or absorber and a persistent oscillator, the inventor proposes to substitute for a single circuit in both transmitter and receiver, a pair of circuits, one of which is so constructed as to radiate or absorb readily, and the other of

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which is so constructed as to oscillate persistently and be a good conserver of energy. The two circuits of the transmitter are tuned together, and linked by means of a transformer in such a way that electrical oscillations in the closed and persistently oscillating circuit build up, and, inasmuch as the primary can act as a reservoir of energy for the secondary, maintain similar oscillations in the open and readily vibrating secondary. Similarly, the two circuits of the receiver, tuned to the same time period as the circuits of the transmitter, are linked through a transformer in such a way that electrical oscillations in the readily absorbing primary build up similar oscil- 11516
lations in a closed and conserving secondary, until such oscillations have strength to break down the coherer.

*   *   *   *   *   *   *

"Take two circuits and let one do one of the things and the other do the other. The electrical engineer reading the specification would thus be led not only to expect a long train of ether waves created through the medium of an open radiating circuit by persistent oscillations occurring in a closed conserving circuit, but an actual increase in the total available energy of the radiating circuit, this increase depending on the conserving qualities of the closed circuit. In other words, the latter would be a reservoir of energy for the 11517
former."

Claim 3 of Tesla #649,621, filed May, 1900, provides:

"3. The combination with a transmitting instrument comprising a transformer having its secondary connected to ground and to an elevated terminal, respectively, the means for impressing in electrical operations upon its primary, of a receiving instrument, comprising a condenser having its primary similarly connected to ground and to an elevated terminal and a translating device connected with its secondary. *The capacity and*

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> *inductance of the two transformers have such values as to secure synchronization with the impressed oscillations.''*

and claim 10 of the Marconi patent in suit:

> ''10. A system of wireless telegraphy in which the transmitting-station and the receiving-station each contains an oscillation transformer, one circuit of which is an open circuit, and the other a closed circuit, *the two circuits at each station being in electrical resonance with each other, and in electrical resonance with the circuits at the other station,* substantially as described.''

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Braun patent #697,544, filed Feb. 6, 1899, has this recital, page 1:

> ''This divisional application relates to apparatus in which the transmitting wire is inductively associated with the oscillation circuit; an oscillating circuit comprising a Leyden jar or inductance coil, is, and for a long time has been, well known to be a persistently oscillating circuit, or a circuit in which, its electrical equilibrium having once been disturbed by vibrations or oscillations, continue for a considerable length of time. In other words, this circuit is a source of maintained or sustained electrical oscillations. This circuit I
> 11520 have likened to a 'reservoir of energy,' a portion of which is radiated from the transmitting wire, *for every oscillation until there is no more energy left,* and the circuit ceases to vibrate.''

While this patent was not issued until 1905, the knowledge disclosed in the application is in advance of the Marconi application; and this is also true of the other contemporaneous applications.

Pupin, in the discussion in New York, at the one hundred thirty-seventh meeting of the American Institute of Electrical Engineers, November 22, 1899, on the pos-

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sibilities of wireless telegraphy, at page 624 of the published report of the proceedings, among other things, said:

> "The forced oscillations of the string are not rapidly decaying, because the body vibrating it—the tuning fork—is a sonorous body. The condenser *a c* (fig. 6) with a shunt, is an electrical oscillator, an electrical tuning fork * * * * * You start oscillations in this circuit just as you start the vibrations of the tuning fork by a stroke. These oscillations then keep up the oscillations in the vertical wire * * * * * * *."

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Mr. Pickard has diagrammatically represented the suggestion, as follows: (See Transcript, Vol. 3, p. 2036 for drawing.)

> "By varying the dimensions of the shunt, *m n o,* or capacity of condenser *E,* we can tune this circuit."

Judge Veeder, in referring to the Marconi patent in suit, in Marconi Wireless Telegraph Co. v. National Electric Co., supra, said:

> "By using two linked circuits in his transmitter, in which the circuit of the primary contains a condenser of any desired capacity, with the usual provision for its discharge through the spark-gap, and in the circuit of the secondary, the vertical wire, any desired energy may be imparted to the radiator, since the closed circuit of the primary is a good *conserver* or *reservoir* of energy for the radiating open circuit of the secondary." 11523

The principle of resonance in telegraphic art may be further shown by patents #640,516, issued January 2, 1900, to Pupin, for "Electrical Transmission by Reso-

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nance Circuits,'' and Pupin patent #519,347, issued in 1894.

Stone, in his patent #577,214, issued February 16, 1897, being ''improvements in resonant electrical circuits,'' assigned to the American Bell Telephone Company, after the circuit is described, says:

> ''Such a circuit is an ideal resonant circuit, and will be highly selective if the coil be of low resistance and great inductance, while the condenser is of small capacity. By the term 'selective' is meant that property which resonant circuits exhibit of responding more strongly to the cur-
> 11525 rents of one particular frequency than to those of any other frequency, and by 'selectivity' reference is made to the degree to which they exhibit this selective property,''

and in Stone patent, assigned to the same company, #638,152, issued November 28, 1899, for ''new and useful improvement in telephony,'' it is said:

> ''In the present invention the high-frequency current is developed by the disruptive discharge of the condenser in the sonorous circuit * * * an arrangement of circuits by means of which the invention may be applied to selective and multiple
> 11526 telephony.''
>
> ''For this purpose the sonorous-resonating circuit * * * * and the resonator-circuit * * * * are atuned to the same frequency, while those at * * * * and * * * * are atuned to some other frequency.''
>
> * * * * * * * *
>
> ''The frequency of this oscillatory current is determined by the electro-magnetic, electrostatic, and dissapative resistance of the primary circuit.''
>
> * * * * * * * *
>
> ''The frequency of the current developed by the transmitting sonorous circuits is determined by the self-inductance and capacity of the circuit, including the primary of the induction-coil, the

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inductance coil, the condenser, and the spark-gap, and by *properly proportioning* this self-inductance and capacity any desired frequency between very wide limits may be obtained. The atuning of the receiving-circuits is likewise accomplished by proportioning the inductance of the secondary circuit to the capacity of the condenser located in the secondary circuit.'' (See Transcript, Vol. 3, p. 2012 for drawing.)

February 28, 1900, Stone disclosed to Mr. Picard, four tuned circuit system, which Mr. Picard has diagrammatically represented as above. 11528

June 30, 1899, prior to conversation with Mr. Picard, Stone wrote to Mr. Baker, in which, *inter alia,* he said:

> ''Instead of utilizing the vertical wire itself at the transmitting-station as an oscillator, I propose to impress upon this vertical wire, oscillations from an oscillator, which oscillations shall be of a frequency corresponding to the fundamental of the wire * * * *. Similarly, at the receiving-station, I shall draw from the vertical wire only that compound of a complex wave, which is of lowest frequency. If, now, the fundamental of the wire at the receiving-station be the same as that of the wire at the transmitting-station, then the receiving-station may receive signals from the transmitting-station, but if it be different from 11529
> that of the transmitting-station, it may not receive those signals.''

In a letter of July 18, 1899, from Stone to Baker, he said:

> ''The tuning of these circuits one to another and all to the same frequency will probably be best accomplished empirically, although the best general proportions may be determined mathematically.''

11530 Opinion.

There is no controversy, I think, as to the authenticity of the letter of July 18th, and I am satisfied that the letter of June 30th was written prior to July 18th, and there is no reason to doubt that it was written on June 30th, the date testified to by the witnesses who have knowledge, although I am satisfied that the date, June 30th, was written subsequent to the writing of the letter.

That resonant transfer of energy was long known to the art is further demonstrated by the fact that as early as May 9, 1894, Morris Hutin and Morris Le Blanc, in application for patent for "Multiple Telegraphy and
11531 Telephony," used this language:

> "It is clear that our electric resonators are circuits in which the real reactance as distinguished from the apparent reactance for the given periodicity of alternating currents is made zero so that the impedance of the circuit is equal to its ohmic resistance. For the purposes of our invention it is often necessary to connect a part of the electric resonators together so as to form a group."
>
> * * * * * * * *
>
> "Supposing, now, that the line, either grounded at each end, as shown, or having a common return-wire, is charged simultaneously with three
> 11532 sets of alternating currents having frequencies 1000, 2000, 3000, respectively, it will be clear that the resonator-circuit *3′*, *4′*, and *5′*, will permit only the circuit having the frequency of, say 1000, to pass through, and that the other resonators will only permit the currents having frequencies of 2000 and 3000, respectively, to pass through. If either of these currents is varied in intensity by any suitable device, at any point on the line, these variations will be felt by the transmitting device in the resonator-circuit only which is adjusted to the frequency of the varied currents."
>
> * * * * * * * *

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> "This analogy between * * * * electrical resonance, we have fully set forth and explained in our articles in *lalumiere Electrique.*"

The prior art publications admitted in evidence present a wealth of information upon the prior art with relation to electrical resonance and tuning, and with the other evidence in the case, remove from my mind any doubt as to these principles with relation to the prior art long before the application for the patent in suit.

The essential necessity of plaintiff's Marconi patent is means to transmit the stored energy of the reservoir-circuit to the antenna circuit, at the rate at which it can 11534
be effectively radiated, and the method by which this energy may be transferred into the radiating circuit, in the same quantity and as it is radiated, is by tuning the primary or persistently-oscillating circuit to the antenna, or radiating circuit, and thereby getting full value, utilizing between the two circuits the principle of resonance. The reservoir-circuit is necessarily always a resonant, i. e., oscillating circuit. The novel difference between the 1896 patent, expired, and the 1900 patent, as given by Marconi, is the removal of the spark-gap from the "open transmitting circuit, and including it in the closed or nearly closed oscillating circuit, which closed oscillating 11535
circuit was inductively connected with the open antenna circuit." In the receiver-circuit the detector is removed from the open antenna circuit with the same relation and upon the same principle.

That tuning is the essence of the Marconi patent in suit is further demonstrated by correspondence between Marconi and the Examiner, upon the rejection of the claim by the Examiner, in which he stated, December 24, 1900, *inter alia*:

> "It is required that further and clearer description be inserted in the specification as to what

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> is meant by 'persistent oscillator' and "good radiator' ",

and the further understanding of the Examiner from correspondence is shown from the following extract from his letter of February 11, 1902:

> "Applicant's argument has been carefully considered, and though difficult of interpretation by reason of grammatical defects, and also because of what seems to be discontinuity of idea, it is correctly or otherwise interpreted by the Examiner to mean that the fundamental of the aerial
> 11537 conductor is a harmonic of the local primary, or
> *vice versa,* and that such a relation necessarily requires that the vibrating body of fundamental periodicity to *have constants such as cause it to be a persistent oscillator and the aerial conductor electrical constants which make it necessarily a good radiator.*"

and counsel for Marconi, in a letter of April 8, 1902, say:

> "It is very plain that the necessity or desirability of tuning the two circuits of the primary to each other or making them accord to each other in their natural period of oscillation is not an obvious suggestion from prior descrip-
> 11538 tions, of desirability of tuning a primary to the
> receiver, as is seen from the fact, although the English patent of Thompson, No. 22020, of 1899, describes a two-circuit primary, yet the inventor makes not the faintest suggestion of the desirability of having the periods of oscillation accord with each other, and although Mr. Marconi himself, in his patent No. 627,650, describes a two-circuit receiving instrument, yet he did not make any suggestion that it would be of advantage to make the periods of oscillation of the two circuits accord."

Opinion. 11539

In determining the construction to be placed upon public documents, consideration is given to departmental interpretation.

June 13, 1903, In re Braun, Application Serial No. 704,505, appears a statement by the patent office prior to the issuance of the plaintiff Marconi patent in suit, and while the application was under consideration, of the meaning of the term "persistent oscillator", in which it is said:

> "Only a definite limited amount of energy can be stored up in any capacity apparatus, and in the
> case of a given oscillator for wireless telegraphy 11540
> it is evident that there is always a fixed maximum charge that cannot be exceeded. If, therefore, an oscillator of given capacity is so constructed as to radiate substantially all of its energy in one or two waves, the one or two waves so radiated will be of the maximum energy possible with such a system, *whereas if the oscillator is so devised as to be a persistent oscillator, it will send out a very much greater number of waves at each discharge, but each wave will have only a fractional amount of the energy that could have been radiated in a single wave.* For instance, in the one case the given capacity being charged with a given amount of energy, *x*, the energy of the one or two waves
> in the one case will be substantially equal to *x*, or 11541
> to *x2*, as the case may be, *whereas in case the radiator is arranged to be a persistent* oscillator so as to give *a long train of waves, as for instance 100,* then the energy *x*, being split up among 100 waves of a train, the energy of each one of those waves will be represented by the value x/100. For use with a coherer, which from its nature operates only in response to the maximum potential impressed at any one instant, the first arrangement would be 100 times more efficient than the latter. It is obvious that if the coherer is so far distant that it will only respond effect-

ively to a wave having the energy, x, at the transmitter, it will not respond to 100 or 1000 or an infinite number of waves, having only the energy x/100."

And Marconi, in his affidavit, when referring to the Braun circuit transmitting apparatus, says:

> "Much less had he (Braun) understood that there was or could be any advantage in making the two circuits of such inductance and capacity as to have a natural period of oscillation in accord with each other."

11543 and referring to the Braun patent, in his deposition, says:

> "Hence the Braun patent illustrates merely the introduction of the transformer without any disclosure of the principles of my patent, namely, the proportioning of the circuits to act as reservoir and radiator respectively, while efficiently transferring the energy by tuning these circuits to one another."
>
> "My patent No. 627,650 differs essentially in principle from my patent No. 763,772, in that in the former I did not describe or disclose the fact that the closed oscillating receiving circuit was to be *tuned to the open* antenna receiving circuit, while in my patent No. 763,772, *I disclose and*
> 11544 *describe means for attaining* this useful object."

and, on page 28 of his deposition, he says:

> "Furthermore this (Fessenden) patent shows no means whereby the tuning could be effected, there being no variable elements shown in either of the circuits."

and on page 31:

> "The difficulties to be overcome in order to transmit an intelligible message to a great dis-

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> tance prior to the utilization of the invention of my patent No. 763,772 were *manifold.* They were also the result of interlinked and obscure causes, and hence one of the greatest difficulties must be overcome to attain success. In the evolution of the apparatus of my patent No. 763,772, I found that I could attain the long distances desired by *separating the functions performed in the original* single wireless transmitting circuit of my patents Nos. 586,193 and Re-issue No. 11,913, and assigning them to associated unlike circuits, one of which was a *closed circuit* and hence not capable of radiating energy, and the other an *open* circuit which was capable of radi- 11546
> ating energy. I found that by doing this I could so construct the closed or non-radiating circuit as to make it serve as a *reservoir* and so perform the function of receiving a relatively large initial charge and that by *properly associating* the circuits and *tuning* them to have the same period, *transfer* this stored energy to the open or radiating circuit and obtain an efficient radiation of a larger quantity of energy with very much less decay of the waves. The transmitting difficulties thus overcome were the *inability to efficiently store* a quantity of energy and the inability to radiate it without undue losses and high damping,"

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It would therefore appear that the definitions given in the specifications and claims to meet the reference made by the patent office, preclude any other conclusion than that tuning was the purpose sought, Sargent v. Hall Safe and Lock Co., 114 U. S. 86; 5 Sup. Ct. 1021, 29 L. Ed. 67; American Stove Co. v. Cleveland Foundry Co., 158 Fed. 983, 86 C. C. A. 182.

The application not being for the principle of resonance or tuning, or such foundation as a scientific fact, reference to specification may be made for interpretation of claims made, Tilgham *v.* Proctor, 102 U. S. 707, 26 L. Ed. 279.

Marconi is not a pioneer in the art of resonance in his invention, in the sense that he discovered the principles of resonance or the art of tuning two circuits to the same frequency. His accomplishment must rest upon the utilization of known principles which he coordinated with ideas of his own invention, which are involved in the patent in issue, and have proven of great value to the world. His patent is not for tuning in general but for particular, variable, selective apparatus,
11549 which the defendant does not use.

> "By way of specific and detailed information as to how the capacity and self-induction of these circuits may be independently adjusted so as to make the product of the self-induction multiplied by the capacity the same in each case, and thus obtain four circuit tuning, a table of tunes is given.
>
> The broad claim of invention resides, therefore, in the independent adjustment of the capacity and self-induction of the four circuits, two at the transmitting-station and two at the receiving-station, so that the product of these elements in each of the two circuits shall be the same, in order that the circuits may be in electrical reso-
> 11550 nance with one another. This broad invention is covered by claims 10 and 20 in issue."
>
> Marconi Wireless Telegraph Co. *v.* National Electic Signal Co., supra.

The Simpson Mercury Valve Transmitter is diagrammatically represented as follows: (See Vol. 2, p. 1080 for drawing.)

D represents an alternating current dynamo, and *T* a power transformer, *V* a mercury vapor rectifying valve, having terminals, *A A1* and *N*. *A* and *A1* are

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anodes, the positive terminals of the valve. *N* is called a cathode, the negative terminal. *R* is resistance, *C*, variable condenser. *2* represents the horizontal structure of the overhead portion of the antenna. *3* is a vertical wire connecting the overhead portion of *2* by variable connection *5*, with the inductance coil *L*, this being a helical coil. *W* is a flat spiral inductance coil, connected to the inner terminal of the inductance coil *L* at the terminus of its center convolution. The extremity of its outer convolution is connected to one terminus of the variable condenser, *C*, and the other terminus of the variable condenser, *C*, connected to the earth by conductor, *4*. *S* is a 11552
spark-gap of special form and is connected to the ground through the conductor *7* and the ground connection wire *4*. The alternating current dynamo is connected through its brushes each to the respective terminus of the primary coil *P C* of the power transformer *T*. *K* is the operator's key. The secondary coil of the power transformer *T* is designated as *S C*. Each outer extremity of this coil is connected to the respective anode of the Mercury Value *V*. From the center of the coil is taken a conductor, *6*, which is connected to the variable condenser, *C*, at the point where it is joined to the extremity of the outer convolution of the spiral *W*. The cathode 11553
end of the Mercury Valve *V* is connected to one extremity of the resistance *R*. The other extremity of the resistance *R* is connected to the variable condenser *C* at its terminal opposite to that connected to the center point of the secondary coil *S C*; the same extremity of the resistance *R* is also connected to the ground through the common ground conductor, *4*. The transmitter functions, as stated by Mr. Simpson and other witnesses, by having dynamo *D* generate an alternating potential current between its brushes, one brush being upon each of the

collector rings, of 110 volts and sometimes 220, depending upon the type or kind of machine. The pressure or potential, of 110, or sometimes 220, is stepped up in the power transformer *T* to a pressure normally 4400 volts between the outer terminals of the secondary coil *SC*, sometimes dropping to 4000 volts. The pressure from the outer terminal to the middle conductor, *6*, is just one-half, or 2200 volts, when it is 4400 volts between the outer extremities of coil *S C*. The Mercury Valve, *V*, will conduct current under normal conditions from the anodes, *A A1*, to the cathode *N*. The current will not flow from the cathode *N* to either anode *A* or
11555 *A1*, unless a relatively high pressure is placed upon the valve. A pressure impressed upon the valve, in the machine in evidence, would be something like 20,000 volts to reverse the current. The direction of the flow of current from the secondary coil *S C*, and the power transformer *T* to the radiating system is always the same. It is a unidirectional current, that can not reverse its direction of flow, because the valve acts as a check valve in the charging system, and permits the current to flow from its source, the secondary coil *S C*, to the point where it is needed. It does not permit the current to be returned from any such point to its source. The
11556 valve *V* has resistance which varies inversely with the current flowing through it. As the current becomes greater in quantity, the resistance becomes less. If the potential between each extreme terminal of the secondary coil *S C* of the power transformer *T*, were in such a direction that if a current were permitted to flow through that coil, from the bottom of the coil, as shown on the chart, to the top of the coil, the Valve *V* would only permit such current to flow from the upper terminal of the secondary coil *S C*, to the anode *A*, thence within the valve to the cathode *N*, thence through the re-

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sistance *R* to the variable condenser *C*, charging the variable condenser *C*, through the conductor *6* back to the center of the secondary coil *S C*. "If we imagine in the next instant of time that the alternating potential had reversed its direction, so that the direction of the alternating potential was such that it would produce a current normally flowing from the top of the secondary coil *S C*, toward the bottom of that coil, then such a current, with the arrangement shown on the chart, would flow from the bottom of the secondary coil *S C*, to the anode *A1* of the valve *V*; thence through the variable con-
denser *C*, and complete its circuit from the variable con- 11558
denser *C*, through the conductor *6* back to the center of the coil again, pulsations being sent in the charging circuit, through the valve *V* into the condenser *C*. These pulsations are also unidirectional, the valve *V* acting as a rectifier, rectifying the alternating current from the dynamo *D*, acting through the transformer *T* and sending unidirectional or pulsating currents into the variable condenser *C*. Now there is also a comparatively small amount of charge taken by the condenser which is constituted by the ground * * * and the overhead portion of the antenna, marked '*2*' on the chart. That amount of energy is really exceedingly small as compared to
the amount of energy placed in that portion of the an- 11559
tenna system, constituted by the variable condenser *C*. The spark-gap *S* is so adjusted as to break down, at some prearranged pressure, or potential. That potential is usually a peak potential of the pulsations,—that is, the highest potential the pulsation reaches. It may even be a little higher than that, due to the so-called inertia effect of the circuit, which would raise it a trifle. This spark-gap *S*, having been adjusted to break down at such a potential, when a sufficient amount of charging current has flowed into the variable condenser *C*, to

11560 bring its potential up to the breakdown point of the spark-gap *S,* a discharge takes place. This discharge is always, with the arrangement of circuits shown in the chart * * * which * * * is identical with the arrangement of circuits in actual use, from the bottom of the condenser *C,* through the conductor *7,* thence through spark-gap *S,* the conductor *8,* and such fraction of the outer convolution of the spiral coil *W* as may be used, thence to the opposite terminal of the variable condenser *C.* That discharge is a unidirectional impulse, and, with a normal arrangement of circuits, i. e.,
11561 the transmitter set up as it always is, for operation, is always a unidirectional impulse. The effect of it is this: Energy is placed within the antenna circuit, the antenna proper of this transmitter, directly from its source.'' The antenna circuit is said to consist of the overhead conductor *2,* its vertical connections *3,* inductance coil, or some portion of inductance coil *L,* flat spiral inductance coil *W,* variable condenser *C,* earth connection *4,* and ground. The spark-gap *S,* with its associated conductors, forms a part of the antenna system. The energy, when communicated from its source into the antenna, is in the static, its potential form, but thereafter is converted from the static, its potential form,
11562 into the kinetic form, that is, into oscillations, within that portion of the antenna consisting of the overhead conductor *2,* conductor *3,* inductance coils *L* and *W*, condenser *c,* earth connection *4,* and ground. Such conversion is known to the art as impulse excitation. Impulse excitation of oscillating currents is one in which the energy is set in motion within the oscillating, radiating circuit, by a single impulse, as distinguished from a transfer of energy by other means.

The quenched spark-gap introduced into this country in 1908 by the Telefunken Company, of Germany, and now used upon all apparatus in suit, has added much to the efficiency of wireless apparatus. By it the character of the wave can be readily controlled, and the quality (sharpness and purity) of the wave can be brought into harmony with the result desired, and the high frequency generator impulse system will enable the operator to hitch "your (his) wagon to the earth and shake it * * * ", and send messages to distant parts of the world, as stated by Pupin, supra.

The action of the grounded antenna does not yet seem to be conclusively determined. Marconi's idea
with relation to the grounded action changed from the 11564
time of the filing of the 1896 application, so that special emphasis to grounded influence was omitted from the 1900 application, but subsequent development confirmed his former belief, and Pupin's suggestion seems to receive emphasis in the high frequency generator impulse and "single chunk" conversion of energy apparatus of defendant.

The purpose of the Simpson Mercury Valve is to prevent an arc to form at the spark *S*, enabling that spark to rapidly regain its high resistance quality and open up in order that it may leave the antenna free to oscillate in its own natural way, and to rectify the alternating
current into a "pulsating current". 11565

The circuit comprised of part of the elements of the antenna system, condenser *C*, and a part of the spiral *W*, across which is placed the spark-gap *S*, making a circuit composed of *S W C 7*, which has the characteristic of having a large ratio of capacity to inductance, and is not a persistently oscillating, generating circuit, but has a function of disturbing the equilibrium of forces which exist in the antenna system, in order that it may produce persistent oscillations of a single frequency in that antenna system. The effect of this circuit upon the radi-

11566 ating circuit, as stated by Dr. Kolster, "is such as to get the energy into that radiating circuit very quickly, and to thereafter allow it to oscillate freely in its own natural way". This circuit has enormous decrement, and is non-persistent. It is not, as in the Marconi patent in suit, *primarily* a reservoir or persistent oscillator, and does not co-operate with the antenna on the principle of resonance.

The defendant, to demonstrate the fact of the "single chunk" conversion of antenna energy, introduced the result of experimentation conducted at the University
11567 of Washington with the Braun tube on the behavior of the Simpson Mercury Valve Transmitter. The photographic reproduction shows the type of photograph obtained by Dr. Kolster, by the use of the Braun tube. The method used was that of producing deflections of the spot of light across the fluorescent screen, and photographing that deflection of light. The deflection of the spot of light across the screen corresponds with the motions of the current, first in one direction and then in the other, and, if the disturbing circuit is not characteristically an oscillating circuit, by the use of the mercury valve, whose function is to rectify the alternating current supply, the spot of light would appear as in the photograph.
11568 If no mercury valve be used, an impulse would be obtained first to the left and then to the right, the time period being a function of the frequency of the generator in a 500 cycle generator supplying the condenser C, occuring at intervals of 1/1000 of a second. The contention of the plaintiff with relation to the Massachusetts test, in which it was shown that there were two and one-half oscillations in the antenna circuit, and that this must refute the contention of the defendants with relation to the Washington University photographic test, may be answered by the suggestion that the Washington Uni-

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versity result was obtained—the photograph speaks for itself—and defendant's witnesses to that extent are corroborated. The Massachusetts experiments show that there were many elements that entered into the experiments with relation to the appliances and the adjustment of the apparatus Dr. Zennick's testimony, which does not seem to be denied, shows that photographs were only taken when the adjustments were such as to produce the desired result, and that the effort was for the purpose of obtaining evidence of oscillations in the trigger circuit, rather than to present to the court the result of
all of the experiments that were made, together with the 11570
adjustments for each result—an impulse charging circuit such as defendant's requires a certain relative inductance and capacity and resistance to produce energy in substantially one oscillation, and the increase of inductance over capacity and resistance beyond the proper ratio will change the characteristic of the impulse charging circuit—and the further fact that it is shown that, as stated by Dr. Zennick, "There was only one system, the dummy antenna, and only one kind of oscillations, the free oscillation of the antenna," and it further appears that oscillatory circuits are not necessarily tuned circuits,
and good tuning is not possible with two and one-half 11571
waves in the train. No facts shown indicate that the oscillations were the result of resonant transfer of energy. Dr. Zennick further stated:

> "I have actually calculated the curves, according to the photographs *B1* and *BM*, assuming the figures given by Dr. Chaffee * * * * and we have length of about 600 metres capacity *CL* equal to .001 microfarads, and the resistance of about 6 ohms, meaning a decrement of about .06. The result is represented on the chart which I marked "*Z7*," assuming as a decrement the value, .04, which had been measured in the tests made at the

11572 Bureau of Standards, and which corresponds to the figures given in the report of the Committee on Standardization of the Institute of Radio Engineers, pages 22 and 23, No. 1011, for a standard antenna of 600 metres we have length * * * * assuming these figures, the corresponding curve is represented on the chart, "*Z8.*" These curves show very clearly that after an extremely small percentage of the entire oscillation time we get the free oscillations of the antenna, if we compare these oscillations represented on charts "*Z7*" and "*Z8*" with the oscillations of the system oscillating
11573 from the beginning with its free oscillations, not only after 2½ oscillations—I have represented these oscillations in figures which are marked "*Z9,*" corresponding to a decrement of .06, and '*Z10*', corresponding to a decrement of .04—it may be that we would hardly become aware of the difference between these figures if our attention had not been called to it. I may add the following: The Committee on Standardization of the Institute of Radio Engineers, in its report for 1915, defendant's Exhibit 61, page 14, has defined 'impulse excitation', as follows: 'A method of producing free alternating current in an excited circuit in which the duration of the exciting circuit is short compared with the duration of the excited
11574 current'. This definition has again been agreed upon in one of the last metings. I fully agree with this definition, and think, therefore, that the oscillations represented in figures "*Z7*" and '*Z8*' represent a very good example of impulse excitation. There is no question that the impulse excitation would be still pure if the free oscillations were already present after one-half oscillations. I have shown this case in figure '*Z11*' for the decrement of .06, but comparing this figure with the corresponding figure '*Z7*', for the same decrement, it seems to me that the difference between these two figures is extremely small. From the standpoint of a physicist, therefore, and consid-

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> ering the result, it seems to me very immaterial whether the free oscillations start after ½ an oscillation or after 2½". (See Vol. 2, p. 1263, for drawing.)

The defendant's Thompson Impulse Transmitter consists of a supply circuit, consisting of an alternating current generator and power condenser, *A*, connected with two resistances, *R R*, which are connected to the opposite terminals of two condensers, *J J*, connected in series with one another. Branching from this condenser *J J* to either side is a symmetrical circuit containing spark-
gaps *H6 H7*, and a single turn of wire marked "*H2*," 11576
this branch circuit being marked "*1*." Inductively associated with the single wire, *H2*, is an antenna circuit consisting of the antenna marked "*2*" and the tuning inductance marked "*H4*," the connecting wire marked "*H*," the coil marked "*H3*," and the connecting wire marked "*H1*," which may go directly to earth, or which may go through a condenser *C* to earth. The circuit containing the condensers *J J* and the spark-gaps *H6 H7*, and the single turn of wire *H2* may be called a disturbing circuit, and consists of the two elements, capacity and inductance, both being fixed in value. The other ele-
ments are so chosen that the ratio of the capacity to 11577
the inductance is extremely large. In the antenna circuit which is inductively associated with the circuit No. *1* of the chart *K8*, the inductance coils *H4* and *H3* have means of varying their inductance by changing the number of turns on either one of the coils or both, in order that this antenna system may be tuned or adjusted to emit any wave length desired between 300 and 600 metres, and particularly two wave lengths 300 metres and 600 metres, which are the two wave lengths for use for commercial purposes.

There being no variable elements in circuit *1*, the only adjustments necessary are in the antenna circuit itself. The tests before the court demonstrated that no change in time period placed more than one wave in the antenna.

In radiating a wave from the antenna of 600 or 300 metres, no variation is made in either the capacity or inductance of the charging circuit, the product of the inductance and capacity in circuit *1* being the same in sending out a 300 metre wave as a 600 metre wave from the antenna. In the operation of this transmitter the only variation in the inductance in changing from 300
11579 to 600 metres is the use of a fewer number of turns in the inductance, and would, in most cases, be made by opening the switch short-circuiting the condenser *C*. The only adjustments are of the antenna circuit for the variation of wave length. The difference in the operation of the defendant's impulse transmitter and the plaintiff type of transmitter is in the fact that there is an entire absence of resonant tuning in the impulse transmitter, whereas in the tuned, coupled circuit transmitter, for any change in wave length there must be a change of tune of the two associated circuits, so as to keep them in resonance. The defendant's impulse transmitter is electrically the same as Lodge's and carries out his
11580 thought with relation to the transmission of energy.

It is conceded by the plaintiff that the defendant Thompson Transmitter is not an infringement of the plaintiff patent when used at 300 metres, but that when used at 600 metres the circuits are in substantial resonance, and because of such fact is an infringement of plaintiff's patent. I do not believe that this contention is tenable. The Thompson Transmitter operates at different wave lengths, as demonstrated before the court, without variation of the time period of the impulse charger, and not upon the principle of resonant transfer. The

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structure of such apparatus substantially differs from plaintiff's, and because its variation is not limited, and at some point it is in harmony with an apparatus with a fixed variation, would not show conflict or infringement, where resonant transfer is not shown. No appliance is placed upon the defendant's apparatus by which tuning to other than fixed frequency can be made by the operator; whereas the plaintiff's apparatus is constructed with variable tuning appliances by which the operator can fix the time period to any desired wave length. The fact that coils of different length may be constructed and
placed upon the Thompson Transmitter, as was demon- 11582
strated by plaintiff before the court, and the time period be thereby varied, cannot avail plaintiff, as such would not be the structure of defendant; nor can the plaintiff be permitted to enlarge the claims of the patent by "broad tuning," in view of the express language employed in the patent. (See Vol. 2, p. 596, for drawing.)

In the Lodge patent in suit, expired, is a radiating system, including coils *h4 h4* and conductors *h h1,* being the antenna and ground connection, respectively, as a radiating circuit connected by way of spark-gaps *h7 h8* and *h6 h8,* to the charging or supply circuit. This circuit consists essentially of two condensers, Leyden jars, *j j*.
To this supply circuit is added the coil, *k*. The intention 11583
is apparent that the spark-gaps *h2 h3* should be gaps of low resistance, and that *j j* should have a high potential; that spark-gap *h10 h11* is called a "starting-gap", and when the spark-gap *h10 h11* breaks down, the charging supply circuit, *jkj*, delivers its charge through the gap *h8 h6* and *h8 h7* to the radiating circuit. When the Rhumkorff coil, *A*, is energized, there is a charge of the condensers, *jj*, until all of the energy which can be stored in these, as limited by the potential reaching above the spark-gap *h10 h11* breaks down, the energy stored in

11584 these condensers *jj* is converted into a sudden or impulsive current rush, first through the circuit *jkj,* and then immediately into an impulsive rush or charge of current across the gaps *h8 h6, h8 h7,* into the conductors *h* and *h1;* and starts an electrical current through the gap *h2 h3* very suddenly, and oscillations are set up in the entire antenna system and radiated. By this system it was not possible to obtain effective tuning or syntony. For effective tuning, resonant tuning, a relatively great number of impulses is required, that is, a drawn out train of
11585 waves. The coils *h4 h4* are variable, so that by changing their value, any desired frequency may be obtained and placed in electrical resonance with any receiving-circuit.

Lodge does not use one circuit as a reservior for the other, there being no resonant transfer of energy, the circuit in which the charge originates being entirely separated when the radiating circuit is charged.

> "A receiver or resonator consists of a similar pair of capacity areas connected by a similarly shaped conductor or self-inductance coil, the whole constituting an absorber arranged so as to have precisely the same natural frequency of electrical vibration as the radiator in use at the correspond-
> 11586 ing remitting station, so that it can accumulate and receive impulses—that is to say, can act accumulatively,—but it must not have a spark-gap such as *H2* and *H3,* or if it have a spark-gap, same must be carefully closed or shunted or bridged across for a good short conductor * * *. Identically the same capacity areas and self-inductance coil can be used at will either as transmitter or as receiver * * * on condition that the 'discharge' spark-gap *H2 H3* of the radiator is perfectly closed whenever acting as receiver." (Lodge patent, page 3, second column).

Lodge has an improvement of the receiving antenna linked through a transformer with a closed circuit containing a coherer, which circuit is not timed to the antenna circuit. The receiver-circuit has in it also a variable coil *h4*. By varying the number of turns in this coil, and hence its inductance, the receiver can be operated in electrical resonance, or tuned to a transmitting-circuit.

For the purpose of furnishing a pure wave, Lodge provides that the energy undelivered to the radiating circuit be cut off, and states, on page 2 of his patent, that "the advantage of this is the charges so communicated
are left to oscillate free from any disturbance due to 11588
maintained connection with the source of electricity," and the maximum effect will be produced on the receiving circuit.

The operation of Lodge patent, fig. 4, is identical with defendant's in using impulse charging and variable inductance coil in the antenna. No reservoir circuit is used, and when the radiating circuit is charged, it is entirely separated from the source of supply circuit which necessarily has some natural period.

The Lodge Receiver consists of a primary tuned antenna circuit, *H H4 H1*. *H4*, in its entirety, is the ver-
tical wire or antenna, and *H1* is the ground connection. 11589
The antenna circuit is tuned to the distant transmitter. It is an oscillating circuit and its inductance and capacity values are so adjusted that its time period is that of the distant transmitting antenna, and the variation adjustment is made by variation of the coil *H4* as shown in figure 13, Receiver. Surrounding coil *H4* is a secondary coil *U*, and connected to the terminals of this secondary coil in the simple series circuit, are the detector, which, as Lodge states, page 4 of his patent, "may be a single point contact" (coherer), and battery, *F*, and the indicator, *G*. The secondary circuit of the Receiver is so associated or linked with the primary or receiving circuit that it does not in any way interfere or hinder the

free oscilation of this receiving antenna. As stated by Lodge, "The idea being thus to leave the resonator free to vibrate electrically without disturbance from attached wires," this being the same as shown in the transmitter in the Lodge patent, figure 4, the idea being to have one single free oscillating circuit at both the transmitter and the receiver. Since coil *H4* is magnetically linked with the secondary coil *U*, the oscillations induce simple oscillations in this coil *U* and its attached circuit. The energy in the antenna circuit of the Receiver is transferred to the secondary circuit, not by being timed or
11591 tuned to the primary or receiving antenna circuit, but by being magnetically linked therewith and induced or forced upon the secondary circuit when any alternating current flows through coil *H4* and a forced vibration by means of this vibration of circuits produced. There are no variable elements in the circuit, hence it cannot correspond in time period or tune with the primary circuit. Lodge gives further details as to the apparatus used in the secondary circuit of this Receiver, coil *U*, indicator *G*, and the battery and detector, *F E*, when he says, In all cases it is permissible, and sometimes desirable, to shunt the coils of telegraphic instruments by means of the resistance or capacity, as shown at *W* in figure *12*." The
11592 operator in the transmitting station shown in figure *4* of *GWP 14*, would first connect to his transmitting series of power, *A*, its storage batteries, and adjust coils *H4 H4* to such values as would tune the circuit to the period of operation corresponding with the distant receiving station with which he wished to communicate, and then operate the telephonic key, causing a short and long series of charging currents from coil *A* to the supply circuit *H6 H8 JJ H8 H7* to flow into the radiating antenna, these impulses being dots and dashes of the Morse Code, the operation of this key being an intermittent operation.

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the intermittances geing simply those of the spark discharging at the starting gaps *H10 H11*. If the Rhumkorf coil or induction coil is employed, the sparks usually occur at the rate of 10 or 20 per second, and the intermittent series of charge currents will flow into the radiator *H H4 H1*. These currents set up oscillations in the radiator corresponding in frequency to the tuning of the circuit to which it has been adjusted. If the distant receiving station hears a signal, it will answer, and so the operator at the transmitter of figure *4* will "listen in." If the transmitting station is correctly adjusted to
the tune of the receiving circuit *H H4 H1*, the passing 11594
wave trains will flow through coil *H4*, force oscillaions upon the secondary circuit, affect the coherer *E* of the circuit, and will transmit the signals from the transmitting circuit, which will be received by the operator. If the signal is weak, the operators can bring the transmitter and receiver into closer tune or resonance. The primary purpose of Lodge seemed to be the removal of the detector and its effect from the receiving antenna. Because of its resistance, it prevented free oscillations of the antenna. (See Vol. 3, p. 1665, for drawing.)

In *GWP Chart 47* is made a comparison between the Lodge patent and the Marconi patents in suit, in which
it is claimed that the receiver, figure 13 of Lodge, is iden- 11595
tical with the defendant's standard receiver. The inductance and capacity values in the antenna circuit are so adjusted that its time period is that of the transmitting antenna, the adjustment being made by the variation of coil *H4*, which is surrounded by the secondary coil, *U*, and connected to the terminals of this secondary coil in a simple series circuit, first, the detector, *E*, being a single point coherer; second, a battery, *F*, and third, an indicator, *G*, the secondary circuit being so associated with the receiving antenna circuit that it does not

11596 in any way disturb the free oscillations, "the idea being thus to leave the resonator free to vibrate electrically without disturbance from attached wires." This arrangement is the same as the transmitter, figure *4* in the Lodge patent, the idea being to leave the transmitting antenna or single oscillating circuit entirely free to vibrate without any disturbance from attached wires, and the same is carried through the entire system of transmitting and receiving. Such receiver is commercially operative, the efficiency of which has been demonstrated before the court. In the right hand lower figure is shown
11597 the Marconi Receiver. Both have a receiving circuit, *1*, containing adjustments for tuning to the wave length of the transmitter—a wide difference appearing in circuit *2*. In defendant's receiver, which is figure *13* of the Lodge patent, there are no adjustments. A fixed coil, *U*, is connected in series with detector, *E*, battery *F*, and indicator, *G;* whereas, in the Marconi patent, there are, in the secondary circuit, a series of adjustments, adjustable inductance *G2*, capacity *H1*, these being the adjustments by which the secondary circuit *2* is operated in resonance or tune with circuit *1*. In the demonstration before the court, it was shown that it was necessary to make ad-
11598 justments in the antenna or receiving circuit, and also to make adjustments of inductance and capacity in circuit No. *2*, while but one adjustment was made in the defendant's Receiver, that being in circuit No. *1*. It is apparent that this receiver has but one tuned circuit, that being a fixed or non-tunable circuit, while the plaintiff's receiver has two tuned circuits, the one being tuned to the other by independent adjustment.

Professor Fleming, in his book, entitled, "The Principles of Electric Wave Telegraphy," 1906 Ed., page 218, in considering inductively coupled circuits such as figure *1* of the patent in suit, says:

> "When two circuits having inductance, resistance, and capacity, are inductively connected together, we are then presented with a unique case to consider, if their natural time periods of oscillations when separate, are the same. Oscillations in one circuit then create a strong response in the other coupled circuit. In practice, we find that this syntony or agreement between the time periods of the two periods must be very exact, if the phenomenon of resonance is to take place."

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In the same book, page 490:

> "There are in fact, only two modes of coupling an open and closed oscillatory circuit, which have any technical value. First, we may couple together the circuits in such a manner that a single pure oscillation, or one single period of vibration is forced upon the aerial or radiator, not its own natural period, but that of the actuating closed circuit. Secondly, we may couple together circuits which have the same free, natural period, when separate, and thus establish a syntonism between the circuits, which, under the condition of a somewhat 'loose coupling' results in the radiation of waves of two different wave lengths." 11601

The tuned relation between the circuits behave as a pair of coupled pendulums, and their interference with each other would produce complex waves, or waves of different lengths, as would the interference of the swinging pendulums with each other produce complex oscillations.

Plaintiff's patent in suit states that any of the prior single circuit or two circuit receivers may be used with the transmitter of the patent, and also deals with a

11602 special form of receiver, in which the capacity and self-inductance of the two circuits are to be independently adjusted, so that the product of self-induction multiplied by the capacity will be the same in the two circuits. Means of independently varying their tune must be provided. In the prior patents, #627,650, #647,007-8-9, and #668,315, referred to, there are no such independent adjustments. In Marconi patent #676,332, a variable inductance-coil is shown in the primary or antenna circuit. While this patent was filed after the date of the filing of the patent in suit, it corresponds, as stated in
11603 the first paragraph of its specifications, on page 1, to Marconi's British patent #5387, dated March 21, 1900, being before the filing of application for patent in suit, and which corresponds to the U. S. patent in issue here.

The question of efficiency or selectivity is not the issue. The issue is infringement. The plaintiff's receiver may be much more efficient and more highly selective, and yet these qualities, of themselves, would not show infringement. There is a distinction between efficiency and selectivity. Efficiency simply means the amount of energy delivered to the detector. The efficiency of a receiver is the energy available at the receiving station, in
11604 the form of passing waves, the amount of energy which is delivered to the detector from such waves. An efficient receiver is one which transmits the greatest amount of energy to the detector, and thereby gives the maximum effect to the indicator. Selectivity is the sharpness of response with reference to transmitting stations of different frequencies or wave lengths. A very selective receiver is one which differentiates sharply between two different transmitting stations having nearly the same wave length or frequency. Defendant's receiver does not include the essential elements of the plaintiff

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receiver. The persistent oscillator, variable condenser, and variable inductance in the detector circuit, are all lacking. There is no relation to tuning.

The mere fact that the same result is obtained by the operation of an apparatus is not conclusive of infringement. Infringement cannot be predicated on results obtained, irrespective of the apparatus employed. The fact that the apparatus of the plaintiff, by "*broad tuning*", and the apparatus of the defendant in normal operation, secure the same result, does not signify infringement, (Goodyear Shoe Manufacturing Co. v.
Spaulding, 101 Fed. 990), the established rule being that 11606
the invention, if any exists, is because of apparatus by which the result is obtained, and not the mode of operation, independent of the mechanical device used, (Westinghouse v. Boyden Power Break Co., 170 U. S. 537, 18 Sup. Ct. 707, 42 L. Ed. 1136.) Results accomplished by mode of operation or function, separate from the means of mechanical devices, is not infringement. Union v. Diamond, 162 Fed. 148, 89 C. C. A. 172. The apparatus (Marconi) in suit is fundamentally different. The structures are different. The mode of operation is dissimilar. Because of such conclusion, it is not necessary to enter into any detailed discussion of the claims
made in the plaintiff's patent, or double patenting, or 11607
any of the other issues raised. As to the apparatus furnished to the United States, this court, in view of the act of 1910 (Act June 25, 1910, c. 423, 36 Stat. 851, [Comp. Stat. 1913, Sec. 9465]), has not jurisdiction, either over a suit for an injunction, of for an accounting. Foster Hose Supporter Co. v. Taylor, 191 Fed. 1003, 111 C. C. A. 667.

I think the prayer of the plaintiff should be denied, except as to the apparatus infringing the Lodge patent, for which an accounting is directed, and unless the par-

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ties can agree as to the amount of damages, the matter will be referred to a Special Master to take testimony and report the same to the court, together with his findings and conclusions. I will set the cause for hearing on the 22nd of December, on the matter of taxation of costs, to afford opportunity to the parties, if they desire to be heard; at which time a form of decree may be presented. (*Italics are mine*)

(Signed) Jeremiah Neterer,
Judge.

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FINAL JUDGMENT. 11611

At a Regular Term of the United States District Court for the Western District of Washington, Northern Division, held in the Federal Building in the City of Seattle, on the 16 day of July, 1917.

HONORABLE JEREMIAH NETERER,
United States District Judge,
Presiding.

This cause came on to be further heard at this term and was argued by counsel: 11612

And thereupon, upon consideration thereof, it was Ordered, Adjudged and Decreed as follows:

1. That letters Patent of the United States issued to Oliver J. Lodge on the 16th day of August, 1898, No. 609,154, during the term thereof, were good and valid in law, as to claims 1, 2 and 5 thereof.

2. That the plaintiff, Marconi Wireless Telegraph Company of America, was the sole and exclusive owner of said Letters Patent No. 609,154 during the term thereof.

3. That the said Oliver J. Lodge was the first, true, 11613
original and sole inventor of the inventions described in said Letters Patent and claimed in the said claims 1, 2 and 5.

4. That the defendant, Kilbourne & Clark Manufacturing Company, during the term of said Letters Patent, and before its expiration, infringed upon said claims 1, 2 and 5 of said Letters Patent, by manufacturing and selling apparatus containing, embodying or employing the inventions of said claims 1, 2 and 5 of said Letters Patent No. 609,154, or material or substantial parts thereof.

5. That the defendant has paid the plaintiff the sum of Four Thousand Dollars ($4,000.00) in full of all claims for damages, gains and profits by reason of the aforesaid infringement of said claims 1, 2 and 5 of said Lodge Patent, including all claims against defendant's customers therefor, but exclusive of claims against the United States of America for apparatus infringing the said Lodge Patent manufactured or sold by the defendant herein for or to the United States of America, as per stipulation of the parties on file herein.

6. That the said patent having expired pending suit,
11615 no injunction shall issue restraining the said infringement.

7. That said Letters Patent of the United States issued to Guglielmo Marconi on June 28, 1904, No. 763,772, for apparatus for wireless telegraphy, are good and valid in law as to claims 1, 2, 3, 6, 8, 10, 11, 12, 13, 14, 16, 17, 18, 19 and 20, when properly construed in connection with the apparatus described in the specifications of said patent, as more fully appears in the opinion herein filed.

8. That the plaintiff, Marconi Wireless Telegraph Company of America, is the sole and exclusive owner of
11616 said Letters Patent No. 763,772.

9. That the said Guglielmo Marconi was the first, true, original and sole inventor of the inventions described in said Letters Patent and claimed in the said claims 1, 2, 3, 6, 8, 10, 11, 12, 13, 14, 16, 17, 18, 19 and 20 thereof.

10. That the manufacture and sale by the defendant of its wireless apparatus known as the "Thompson Transmitter," the "Simpson Transmitter," and its Standard Receiver, or either of them, as set forth, shown and described in the proofs herein, does not infringe upon the inventions described in said Letters Patent No.

 11617

763,772, and claimed in said claims 1, 2, 3, 6, 8, 10, 11, 12, 13, 14, 16, 17, 18, 19 and 20 thereof, or any of them.

11. In view of the Act approved June 25, 1910, entitled "An Act to Provide Additional Protection for Owners of Patents of the United States and for Other Purposes," the Court has no jurisdiction to grant the prayer of the bill for an injunction and an accounting in respect to the wireless telegraph apparatus manufactured and sold by the defendant to the United States Government, a set of which was installed on the Steamship "Achilles," and the Court, therefore, makes no finding upon the ques-
tion of whether said apparatus infringes the patent in 11618
issue.

12. That the prayer of the bill, so far as it seeks an injunction and an accounting in respect to defendant's said wireless telegraph apparatus installed on the Steamship "Achilles" as an infringement of each of the aforesaid claims of the said Lodge Patent No. 609,154 and Marconi Patent No. 763,772, and so far as it relates to an injunction and an accounting in respect to the defendant's aforesaid "Thompson Transmitter," "Simpson Transmitter," and its Standard Receiver as infringements of each of the aforesaid claims of Marconi Patent
No. 763,772, be and hereby is denied, and the bill of 11619
complaint, so far as it relates to such apparatus as infringements of said claims be and hereby is dismissed.

14. That no costs or disbursements heretofore incurred by either plaintiff or defendant in this proceeding be allowed or taxed in favor of either party hereto.

To paragraphs No. 10, 11 and 12 hereof, and each thereof, the plaintiff excepts, and said exceptions are hereby allowed.

JEREMIAH NETERER,
United States District Judge.

11620 ORDER.

At a regular session of the above entitled Court, during the May, 1917, Term of said Court, it is hereby ORDERED that said May, 1917, Term of said Court be and the same hereby is extended to and including the first day of February, 1918, for the purposes of taking such proceedings and the making and entering of such orders as may be necessary or proper in connection with an appeal or other manner of review of the judgment in this cause, and for such other purposes in said cause as may be deemed necessary or proper; and it is expressly hereby ordered that the time within which to file, settle
11621 and certify a bill of exceptions or other statement of the evidence herein be and the same hereby is extended to and including said first day of February, 1918.

Done in open Court this 31st day of October, 1917.

JEREMIAH NETERER,
Judge.

11622

PETITION FOR APPEAL. 11623

To the Honorable Judges of the District Court of the United States for the Western District of Washington, in the Ninth Circuit:

The above-named plaintiff, the Marconi Wireless Telegraph Company of America, feeling itself aggrieved by so much and such parts of the decree entered herein this 18 day of July, 1917, as refuses an accounting and injunction and dismisses the bill in respect to claims 1, 2, 3, 6, 8, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19 and 20, issued to Guglielmo Marconi, on June 28th, 1904, of Letters Patent No. 763,772 for Apparatus for Wireless Telegraphy, and feeling itself aggrieved by so much and such 11624
parts of said decree as refuses an accounting and dismisses the bill in respect to wireless telegraph apparatus manufactured and sold by the above-named defendant, the Kilbourne & Clark Manufacturing Company to the United States Government, as infringement of claims 1, 2 and 5 of Letters Patent in suit issued to Oliver J. Lodge on August 16th, 1898, No. 609,154, for Improvements in Electric Telegraphy, does hereby appeal to the United States Circuit Court of Appeals for the Ninth Circuit from so much and such parts of said decree for the reasons specified in its assignment of errors filed herewith; and it prays that this appeal may be allowed, 11625
and that a citation be granted, directed to the above-named defendant, the Kilbourne & Clark Manufacturing Company, commanding it to appear before said United States Circuit Court of Appeals, and to do and receive what may appertain to justice to be done in the premises; and that a transcript, duly authenticated, of so mucn of the record, exhibits, and proceedings herein as pertains to the subject matter of this appeal, may be transmitted

11626

with this appeal to the said United States Circuit Court of Appeals.

L. F. H. Betts,
E. C. Hughes,
Plaintiff's Solicitors.

Sheffield & Betts,
Hughes, McMicken, Ramsey & Rupp,
Of Counsel.

Dated, this 19th day of Nov., 1917.

The foregoing petition for appeal is allowed as
11627 prayed for, and the Clerk of this Court is directed to certify according.

Nov. 19, 1917.

(Sgd.) Jeremiah Neterer,
United States District Judge.

Copy of within petition for appeal and order received and due service of same acknowledged this 19th day of November, 1917.

E. L. Skeel-Donworth & Todd & Phillip Farnsworth,
Solicitors for Defendant.

11628

ASSIGNMENT OF ERRORS. 11629

And now comes the above named plaintiff, by its solicitors, and presents with its accompanying petition for appeal from certain parts of the decree herein on the 18th day of July, 1917, the following assignment of errors:

The Court was in error:

1. In its interpretation or construction of the specifications and drawings of the Letters Patent in suit issued to Guglielmo Marconi on June 28, 1904, No. 763,772, on application filed November 10, 1900, for Apparatus for Wireless Telegraphy, and particularly so far as said 11630
specification and drawings relate to the inventions of the claims in issue numbered 1, 2, 3, 6, 8, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19 and 20 thereof.

2. In disregarding or not considering any of the aforesaid claims of said patent No. 763,772 in determining the character and scope of the said inventions thereof.

3. In holding that none of the said inventions of the said Letters Patent No. 763,772 was of a primary or broad character.

3a. In failing to give any of said inventions of said patent No. 763,772 the benefit of a reasonable range 11631
of equivalents or a range of equivalents commensurate with said inventions.

4. In giving all of the said inventions of said patent No. 763,772 a more limited interpretation or construction than that required by the terms of the aforesaid claims in issue, the specifications of said patent, the actual inventions thereof, the state of the prior art bearing thereon, and the evidence herein.

5. In holding that each and all of the said inventions of said patent No. 763,772 were limited to the specific apparatus shown in the drawings of said Letters Patent.

6. In holding that all of said inventions of said patent No. 763,772 were limited to particular variable, selective apparatus, or to tuning wireless telegraph circuits by the specific apparatus shown in said patent.

7. In holding that in the said inventions of said patent No. 763,772, means of independently varying the tune must be provided.

8. In holding that the plaintiff's contention that said patent No. 763,772 covered broad tuning was an enlargement of said inventions thereof.

11633 9. In holding that the said inventions of the said patent No. 763,772 were limited to perfect tuning or resonance.

10. In holding that "an essential element" of the primary and secondary circuits of the said inventions of the said patent No. 763,772 was a variable means of adjustment in each circuit.

11. In holding that the primary circuit of the transmitter and the secondary circuit of the receiver of the said inventions of said patent No. 763,772 were "persistently oscillating" circuits or must be limited to such circuits.

11634 12. In considering or giving weight to the definition of "Resonant" as found in the Standard Dictionary of 1895, and applying such definition to the term as used in the art of wireless telegraphy in 1900 and in said patent No. 763,772.

13. In its findings as to the mode of operation of the apparatus of the said inventions of said patent No. 763,-772.

14. In holding that the purpose of the primary circuit of the said inventions of the apparatus of the said patent No. 763,772 was to "slowly furnish" stored energy to replace that radiated through the antenna circuit.

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15. In holding that the receiver of the said patent No. 763,772 "has two circuits of like characteristics."

16. In holding that a "persistent oscillator" and a persistently oscillating circuit were the same in mode of operation or function.

17. In holding that the "expression 'persistent oscillator' is neither used nor defined in the specificataion" of Marconi's British patent No. 7,777.

18. In considering and giving weight to, and interpreting and construing the said patent No. 763,772, and particularly the meaning of the term "persistent oscillator" by the statements and arguments of Patent Office 11636
officials, in rejecting or passing upon applications for patents by others than Marconi, and particularly in June, 1903, in passing upon the Braun application Serial No. 704,605, patent No. 797,169.

19. Because its finding as to the scope of the said inventions of said patent No. 763,772 is against a preponderance of the evidence, and contrary to the weight of evidence and contrary to law.

20. In giving any material weight, in determining the scope of the said inventions of said patent No. 763,772, to the theory of the conducting functions of the earth in transmitting wireless telegraph signals. 11637

21. In considering the possibilities, suggestions, and prophesies contained in prior publications, and particularly those made by Prof. Pupin, as material or relevant in determining the state of the prior art bearing upon or affecting the scope of the said inventions of said patent No. 763,772.

22. In limiting the scope of the said inventions of said patent No. 763,772, by, or in giving any material weight to, the drawings made at or immediately prior to the time of the trial by defendant's expert Pickard of the

suggestions, possibilities and prophesies of prior publications, and the defendant's testimony concerning the same.

23. In limiting the scope of the said inventions of said Marconi patent No. 763,772 by the Marconi reissue patent No. 11,913, Marconi patents Nos. 627,650, 647,007, 647,008, 647,009, Tesla patent No. 649,621, the Tesla book by Martin, the Pupin Article or Lecture at the American Institute of Electrical Engineers on November 22, 1899, the Standard Dictionary of 1895, and "The Principles of Wireless Telegraphy" by Fleming, or by either or any
11639 of said patents or publications.

24. In limiting the scope of the said inventions of said patent No. 763,772, by Prof. Fleming's statements in "The Principles of Electric Wave Telegraphy" 1906 edition, the statements of Mr. Marconi in his Nobel Prize lecture, and the statements of Mr. Marconi and Prof. Pupin in the "Transactions of the New York Electrical Society Paper" of April 17, 1915, or by either or any of said statements.

25. In limiting the scope of the said inventions of said patent No. 763,772, by the Marconi United States Letters Patent Nos. 668,315, 676,332, and by the Marconi
11640 British patent No. 5387 of 1900, and the Braun patents Nos. 797,169 and 797,544, issued subsequent to the filing of the application for, and the date of the said inventions of said patent No. 763,772, or by either or any of said alleged prior patents.

26. In limiting the scope of the said inventions of said patent No. 763,772 by, and in giving any material weight to, the recitals or statements inserted in 1902 and 1903 in the applications for the Braun patents Nos. 797,169 and 797,544, or of either of said patents, issued subsequent to the filing of the application for, and the date of the said inventions of said patent No. 763,772.

27. In holding that the knowledge disclosed by said recitals or statements of said Braun applications and "other contemporaneous applications" was "in advance of the Marconi application" in point of fact or law.

28. In limiting the scope of the said inventions of said patent No. 763,772, by, and in giving any material weight to, the several patents and publications relating to wire telegraphy or wire telephony, and particularly patents to Pupin No. 519,347 and 640,516, Stone 577,214 and 638,152, and the Hutin and LeBlanc patent No. 838,-544, issued subsequent to the filing of the said patent No.
763,772. 11642

28a. In admitting in, and considering as evidence, the said alleged prior patents to Braun Nos. 797,169 and 797,544, to Hutin and LeBlanc No. 838,544, and Marconi British patent No. 5387 of 1900.

29. In considering the alleged disclosures by Mr. Stone to Mr. Pickard, or Mr. Stone to Mr. Baker, or the drawings made by Mr. Pickard, at or immediately preceding the trial, of the apparatus or system of said alleged disclosures, or in giving any material weight to said alleged disclosures or drawings as part of the prior art bearing upon, limiting, or anticipating any of the
said inventions of said patent No. 763,772. 11643

30. In holding that on February 28, 1900, John Stone Stone made any disclosure within the meaning of the Patent Law to defendant's expert Mr. Pickard, of "four tuned circuit system."

30a. In giving any material weight as matter of, and in view of, the Patent Law, to the said alleged disclosures of said John Stone Stone.

31. In holding that the alleged letter dated "June 30, 1899" from Mr. Stone to Mr. Baker was written on

that date or prior to the date of the said inventions of said patent No. 763,772.

32. In holding that the principle of resonance was old in the art as practically applied to wireless telegraphy.

33. In holding that "the prior art publications admitted in evidence" prior to the application for said patent No. 763,772 "presented a wealth of information" in relation to the application or practical application to wireless telegraphy as disclosed in said patent, of the principles of resonance or tuning.

11645 34. In holding that the question of infringement of the aforesaid claims of said patent No. 763,772 by the defendant's manufacture, use or sale of the "Simpson Mercury Valve Transmitter" was within the issue presented by the pleadings or had been tendered by the plaintiff.

35. In compelling the plaintiff to try the question of the infringement of said claims of said patent No. 763,772 by defendant's manufacture, use or sale of the said Simpson Mercury Valve Transmitter.

36. In admitting in evidence for the defendant, in relation to the Simpson Mercury Valve Transmitter, the
11646 bill of complaint and affidavits of John Bottomley, David Sarnoff, Roy A. Weagant and Frank N. Waterman, filed on behalf of the plaintiff on the motion for preliminary injunction in the suit of the plaintiff against the American-Hawaiian Steamship Company in the United States District Court for the Eastern District of New York.

37. In admitting in evidence for the defendant, in relation to the Simpson Mercury Valve Transmitter, the bill of complaint and the affidavits of George S. De Sousa, Worth M. Chatfield and Frank N. Waterman, filed on behalf of the plaintiff on the motion for preliminary

injunction in the suit of the plaintiff against the Detroit & Cleveland Navigation Company in the U. S. District Court for the Western District of New York.

38. In the admission and consideration of any of the defendant's evidence and exhibits relating to the construction and mode of operation of the said Simpson Mercury Valve Transmitter, when no evidence thereof had been offered by the plaintiff.

39. In the admission and consideration of any of the defendant's evidence and exhibits relating to the alleged non-infringement of the said claims of said patent No. 11648
763,772 by the manufacture and sale of the Simpson Mercury Valve Transmitter, when no evidence relating to the question of infringement by such apparatus had been offered by the plaintiff.

40. In denying plaintiff's motion to strike out the defendant's said evidence in relation to said Simpson Mercury Valve Transmitter.

41. In admitting in evidence on the part of the defendant, testimony as to tests made of said Simpson Mercury Valve Transmitter at the University of Washington on March 12 and 13, 1916, because said apparatus, in construction and mode of operation, did not conform to the 11649
said Simpson Mercury Valve Transmitter alleged to have been manufactured or sold by the defendant prior to the filing of the bill herein.

42. In admitting in evidence, after the defendant had closed its replying case, and while plaintiff was offering its rebuttal evidence, the testimony of defendant's witness Simpson relating to the date of complete manufacture by the defendant of a Simpson Mercury Valve Transmitter.

43. In holding that the defendant had not infringed upon or contributed to the infringement of any of the

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aforesaid claims in issue of said patent No. 763,772, by the manufacture, offer for sale, sale, installation or use of said Simpson Mercury Valve Transmitter.

44. Because its finding of non-infringement of any of the aforesaid claims of said patent No. 763,772 by the manufacture, use, sale or installation of said Simpson Mercury Valve Transmitter is against a preponderance of the evidence and contrary to the weight of the evidence and contrary to law.

45. In failing to consider or give any weight to Defendant's Exhibit No. 10, Report of the United States
11651 Bureau of Standards of tests of said Simpson Mercury Valve Transmitter conducted by and under the supervision of defendant's witness, Mr. Kolster, and in failing to consider any of plaintiff's and defendant's evidence relating to said tests or the said report.

46. In its findings as to the construction and mode of operation of defendant's Simpson Mercury Valve Transmitter, "Thompson Transmitter" and "Standard Receiver."

47. In holding that in the said Simpson Mercury Valve Transmitter "the spark gap S with its associated circuits forms part of the antenna system."

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48. In holding that "impulse excitation of oscillating current is one in which the energy is set in motion within the oscillating radiating circuit by a single impulse as distinguished from a transfer of energy by other means."

49. In holding that by the quenched spark gap "the character of the wave can readily be controlled and the quality (sharpness and purity) of the wave can be brought into harmony with the result desired" and that the "high frequency generator impulse system will enable the operator to hitch "your (his) wagon to the earth

and shake it . . .' and send messages to distant parts of the world as stated by Pupin."

50. In holding that one of the purposes or functions of the mercury valve in said Simpson Mercury Valve Transmitter was to "leave the antenna free to oscillate in its own natural way."

51. In holding that the primary circuit of said Simpson Mercury Valve Transmitter was "not a persistently oscillating generating circuit."

52. In holding that the primary circuit of said Simpson Mercury Valve Transmitter has the "function of 11654
disturbing the equilibrium of forces which exist in the antenna system in order that it may produce persistent oscillations of a single frequency in that antenna system."

53. In holding that the primary circuit of said Simpson Mercury Valve Transmitter is not "primarily a reservoir or persistent oscillator," or "does not co-operate with the antenna on the principle of resonance."

54. In admitting in evidence "F. G. S.-4" of Defendant's Ex. No. 16, an alleged photograph of the results of defendant's experiments and tests of said Simpson Mercury Valve Transmitter conducted at the Washing- 11655
ton University, and in giving any weight to said photograph on the question of the mode of operation of said transmitter and its infringement.

55. In holding that said alleged photograph "F. G. S.-4" "speaks for itself."

56. In giving any weight to the said alleged photograph "F. G. S.-4", and the testimony of any of defendant's witnesses as to said tests and experiments with said Simpson Mercury Valve Transmitter at the Washington University, conducted in the absence of, and with-

11656 out the knowledge of plaintiff's counsel, experts or engineers, or any invitation to any of them to witness the same.

57. In holding, against the weight of evidence, and contrary to the evidence of Defendant's Exhibit No. 10, the testimony of defendant's witnesses Pickard, Kolster and Zenneck, and plaintiff's witnesses Chaffee, Morecroft, Cross, Waterman and Weagant, that said alleged photograph "F. G. S.-4" and defendant's evidence relating thereto and to the tests of said Simpson Mercury Valve Transmitter conducted at the University of Wash-
11657 ington, established the mode of operation of said transmitter.

58. In not giving due consideration and weight to the plaintiff's evidence of the Massachusetts tests of said Simpson Mercury Valve Transmitter and particularly as establishing oscillations, or a material number of oscillations, in the primary circuit of said Simpson Mercury Valve Transmitter.

59. In not holding that the evidence of plaintiff established that the defendant's Washington University tests of said Simpson Mercury Valve Transmitter were useless.

11658 60. In failing to consider or give any weight to plaintiff's evidence of Messrs. Chaffee, Morecroft, Cross, Waterman and Weagant, that defendant's Washington University tests of the said Simpson Mercury Valve were useless.

61. In holding that Mr. Zenneck's testimony "shows that photographs" (of Massachusetts tests) "were only taken when the adjustments were such as to produce the desired result" and that "the effort was for the purpose of obtaining evidence of oscillations in the antenna circuit rather than to present to the Court the results of all

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the experiments that were made, together with the adjustments for such results'', and that such testimony did not seem to be denied.

62. In holding that good tuning is not possible with 2½ oscillations in the primary circuit or 2½ waves in the train of the primary circuit.

63. In holding that the evidence of the Massachusetts tests of said Simpson Mercury Valve Transmitter showed no facts indicating that the oscillations in the antenna circuit of the said Simpson Mercury Valve Transmitter were the result of resonant transfer of energy. 11660

64. In its interpretation of the operation of said Simpson Mercury Valve Transmitter, as represented in the curves of such operation made and produced by the witness Zenneck.

65. In refusing to admit evidence tendered on the part of the plaintiff, and particularly through the witnesses O'Farrell and Weagant, that defendant's ''Thompson Transmitter'', installed on the vessels Dora, Tyee and Pioneer, was prior to the beginning of this suit, so constructed and arranged as to be tuned at or about a 300 meter wave length; that subsequently and pending this suit such transmitter was changed or altered so that the arrangement and adjustment for tun- 11661
ing to said 300 meter wave length, as well as to a 600 meter wave length, were concealed, and that such evidence did not come to the knowledge of the plaintiff until shortly before such evidence was tendered by the plaintiff.

66. In refusing to admit by the cross-examination of the defendant's witness Wolff, evidence that the defendant's wireless telegraph apparatus in issue had means for varying wave lengths.

11662 67. In holding that the defendant had not infringed upon or contributed to the infringement of any of the aforesaid claims in issue of patent No. 763,772, by the manufacture, sale, installation or use of the said "Thompson Transmitter."

68. Because its finding of non-infringement of any of the aforesaid claims in issue of said patent No. 763,772, by the manufacture, sale, use or installation of the Thompson Transmitter is against a preponderance of the evidence, and contrary to the weight of the evidence and contrary to law.

11663 69. In holding that in the operation of the Thompson Transmitter the only variation in inductance in changing from 300 to 600 meters is in the use of a fewer number of turns in the inductance and that this is usually done by opening the condenser switch.

70. In holding that in the Thompson Transmitter there is an entire absence of resonant tuning.

71. In holding that "in the tuned coupler circuit transmitter there must be a change of tune of the two associated circuits."

72. In holding that the Thompson transmitter is electrically the same as Lodge and carries out the Lodge
11664 idea with relation to the transmission of energy.

73. In holding that in the Thompson Transmitter resonant transfer of energy was not shown.

74. In holding that Thompson Transmitter is like Lodge Transmitter in having no reservoir circuit.

75. In holding that the Thompson Transmitter operates at different wave lengths without variation of the time period of the "impulse transmitter" and not upon the principle of resonant transfer.

76. In holding that no appliance is placed upon the

said Thompson Transmitter by which tuning to other than fixed frequency can be made by the operator.

77. In misapprehending the evidence of the plaintiff as to the effect of applying coils of different lengths to the primary of the Thompson Transmitter.

78. In holding that substitution of coils in the Thompson Transmitter is not a proper method of showing the effect of what defendant does use in said transmitter.

79. In holding contrary to the evidence that in the
apparatus of the Lodge patent No. 609,154, "the energy 11666
stored in the condenser jj is converted into a sudden or impulsive rush."

80. In not holding that the said Lodge patent shows a split antenna system which has no impulsive rush and which radiated two waves.

81. In holding that the operation of the apparatus of said Lodge patent Fig. 4 was "identical with defendant's."

82. In holding that the Thompson Transmitter is the same in construction and mode of operation as the apparatus of Fig. 4 of said Lodge patent.

83. In holding that defendant's receiver was the 11667
same in construction and mode of operation as the apparatus of Fig. 13 of said Lodge patent.

84. In holding that the receiver of Fig. 13 of said Lodge patent is commercially operative.

85. In holding that any receiver or any apparatus of the Lodge Fig. 4 and 13 of the said Lodge patent or Tesla patent were demonstrated before the Court.

86. In failing to consider or give due weight to the evidence of the tests conducted by the plaintiff with the

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alleged apparatus of Figs. 4 and 13 of Lodge patent No. 609,154 in the presence of defendant's experts and Assessors appointed by the Court.

87. In refusing to admit evidence tendered by the plaintiff, particularly through witness O'Farrell, as to the operation and effectiveness of said defendant's "Standard Receiver" at wave lengths of over 1600 meters or thereabouts under commercial conditions.

88. In holding that the defendant had not infringed upon or contributed to the infringement of any of the aforesaid receiver claims in issue of said patent No.
11669 763,772, by the manufacture, sale, installation or use of defendant's "Standard Receiver."

89. Because its findings of non-infringement of any of the aforesaid receiver claims in issue of said patent No. 763,772, by the manufacture, use, sale or installation of defendant's "Standard Receiver" are against a preponderance of the evidence, and contrary to the weight of the evidence, and contrary to law.

90. In holding that said defendant's "Standard Receiver" has but one tuned circuit, that being a fixed or non-tunable circuit.

11670 91. In holding that the defendant's "Standard Receiver" "does not include the essential elements of plaintiff's receiver. The persistent oscillator, variable condenser, and variable inductance in the detector circuit are all lacking. There is no relation to tuning."

92. In giving any material weight to any evidence on the part of the defendant as to tests or experiments conducted with the said Simpson Mercury Valve Transmitter, said Thompson Transmitter and the Defendant's Receiver, in the absence of the plaintiff, its engineers or experts, and without any invitation to the plaintiff, its engineers or experts, to be present at said tests or experiments.

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93. In failing to consider or give due weight to the evidence of the tests conducted by the plaintiff of the said Thompson Transmitter and Standard Receiver, in the presence of defendant's counsel, engineers and experts, and the Assessors appointed by the Court with the consent of both parties.

94. In holding that the apparatus of the Marconi patent and its mode of operation are fundamentally different from the Simpson Mercury Valve Transmitter, the Thompson Transmitter, and defendant's Standard Receiver, and their respective modes of operation.

95. In disregarding or not considering any of the 11672
aforesaid claims in issue of the said patent No. 763,772 in determining the question of infringement thereof.

95a. In limiting all of the said inventions of said patent No. 763,722, and in holding that when so limited, none of said inventions had been infringed by the defendant.

96. In permitting cross examination of plaintiff's witness Weagant, as to whether or not he agreed with statements of Mr. Marconi in his Nobel Prize Lecture.

97. In permitting cross examination of plaintiff's expert, Mr. Waterman, on plaintiff's prima facie case, as 11673
to the construction and mode of operation of the alleged infringing apparatus involved in the suit of this plaintiff against the DeForest Radio Telephone & Telegraph Company, and the infringing apparatus involved in the suit of this plaintiff against the Atlantic Communication Company for the infringement of said patent No. 763,-772.

98. In admitting in evidence on the part of the defendant testimony as to the construction and mode of operation of the infringing apparatus involved in the suit of this plaintiff against the Atlantic Communica-

tion Company for the infringement of said patent No. 763,772.

99. In refusing to admit evidence tendered by the plaintiff as to the construction and mode of operation of the infringing apparatus involved in said suit of this plaintiff against the Atlantic Communcation Company.

100. In denying plaintiff's motion to strike out defendant's exhibits contained in Binder C (Defendant's Exhibit No. 38) relating to Telefunken wireless telegraph apparatus involved in the said suit of this plaintiff against the Atlantic Communication Company.

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101. In denying a recovery of the profits and savings made by the defendant and damages sustained by the plaintiff by reason of defendant's infringement or contributory infringement of the aforesaid claims in issue of said patent No. 763,772, or any of them, by defendant's manufacture, sale, installation or use of any of the wireless telegraph apparatus known as the "Simpson Mercury Valve Transmitter", the "Thompson Transmitter" and "Defendant's Standard Receiver," or any of such apparatus.

102. In denying an injunction restraining the defendant from infringing upon and contributing to the in-
11676 fringement of the aforesaid claims in issue of the aforesaid patent No. 763,772, or any of them, by defendant's manufacture, sale, use or installation of the wireless telegraph apparatus known as the "Simpson Mercury Valve Transmitter", the "Thompson Transmitter", "Defendant's Standard Receiver", or any of such apparatus.

103. In not holding that the wireless telegraph apparatus manufactured and sold by the defendant to the United States, of the character installed upon the SS. "Achilles", embodied the inventions of any of the said

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claims in issue of the said Lodge Patent No. 609,154, and the said claims in issue of the Marconi Patent No. 763,-772, in view of the failure of the defendant to introduce any evidence tending to establish that said apparatus did not embody the inventions of any of said claims.

104. In not holding that the wireless telegraph apparatus manufactured and sold by the defendant to the United States, of the character installed upon the SS. "Achilles", was an infringement or embodiment of any of the said claims in issue of the said Lodge Patent No. 609,154.

105. In not holding that the wireless telegraph apparatus manufactured and sold by the defendant to the United States, of the character installed upon the SS. "Achilles", was an infringement or embodiment of any of the said claims in issue of the said Marconi Patent No. 763,772. 11678

106. In holding that it had no jurisdiction to grant the prayer of the bill for an injunction and accounting as to said claims in issue of said Letters Patent No. 763,772, or any of them, by the defendant's manufacture and sale of wireless telegraph apparatus to the United States, and of the character installed upon the SS. "Achilles". 11679

107. In holding that it had no jurisdiction to grant the prayer of the bill for an injunction and accounting as to said claims in issue, or any of them, of the said Lodge Patent No. 609,154, by defendant's manufacture and sale of wireless telegraph apparatus to the United States, and of the character installed upon the SS. "Achilles".

108. In denying an injunction and an accounting of profits and damages, and in dismissing the entire bill

as to said wireless telegraph apparatus manufactured and sold by the defendant to the United States.

109. In dismissing the bill as to said Letters Patent No. 763,772.

L. F. H. Betts,
E. C. Hughes,
Solicitors for Plaintiff.

Sheffield & Betts,
Hughes, McMicken, Ramsey & Rupp,
Of Counsel.

11681

Service of this Assignment of Errors and a copy thereof is hereby admitted this 19th day of November, 1917.

E. L. Skeel, Donworth & Todd & Philip Farnsworth,
Solicitors for Defendant.

11682

BOND. 11683

KNOW ALL MEN BY THESE PRESENTS: That the plaintiff, the Marconi Wireless Telegraph Company of America, a corporation organized under and by virtue of the laws of the State of New Jersey, as principal, and United States Fidelity and Guaranty Company, a corporation organized and existing under and by virtue of the laws of the State of Maryland and authorized to transact the business of surety in the State of Washington, as surety, executing this bond in behalf of said principal, are held and firmly bound by the above named Kilbourne & Clark Manufacturing Company, a corporation organized and existing under and by virtue of the laws of the State of 11684
Washington, in the sum of Five Hundred Dollars ($500.00), to be paid to the said Kilbourne & Clark Manufacturing Company, its successors or assigns, for the payment of which, well and truly to be made, the said Marconi Wireless Telegraph Company of America and United States Fidelity and Guaranty Company bind themselves and their respective successors and assigns firmly by these presents.

Sealed with our seals and dated this 19th day of November, 1917.

WHEREAS, the above named Marconi Wireless Telegraph Company of America has prosecuted, or is about 11685
to prosecute, an appeal to the United States Circuit Court of Appeals for the Ninth Circuit, to reverse, modify or correct certain parts of the decree entered on the 18th day of July, 1917, in the above entitled suit;

NOW, THEREFORE, the condition of this obligation is such that if the above named Marconi Wireless Telegraph Company of America shall prosecute the said appeal to effect and answer all costs, if it shall fail to sustain its said appeal, then this obligation shall be void; other-

11686 Bond.

wise, the same shall be and remain in full force and virtue.

MARCONI WIRELESS TELEGRAPH COMPANY
OF AMERICA,
SHEFFIELD & BETTS,
By E. C. HUGHES.

UNITED STATES FIDELITY AND GUARANTY
COMPANY,
By GROVER C. WINN,
[SEAL.] Its Attorney in Fact.

11687

Service of this Bond and a copy thereof is hereby admitted this 19th day of November, 1917.

E. L. SKEEL, DONWORTH & TODD &
PHILIP FARNSWORTH,
Solicitors for Defendant.

The above bond and the surety thereon is approved this 19th day of November, 1917.

(Sgd.) JEREMIAH NETERER,
Judge.

11688

CITATION. 11689

United States of America, ss.:

The President of the United States, to
Kilbourne & Clark Manufacturing Company:

You are hereby cited and commanded to appear at a session of the United States Circuit Court of Appeals for the Ninth Circuit to be held in the City of San Francisco, California, in said Ninth Circuit, on the 17th day of December, 1917, pursuant to an appeal duly allowed and filed in the Clerk's office of the District of the United States for the Western District of Washington,
wherein the Marconi Wireless Telegraph Company of 11690
America is the Appellant and you are the Appellee, to show cause, if any there be, why certain parts of the decree entered in said Court on the 18th day of July, 1917, should not be reserved, modified or corrected, and why speedy justice should not be done to the parties in that behalf.

Witness the Honorable Jeremiah Neterer, United States District Judge for the Western District of Washington, this 19th day of November, 1917, and the Independence of the United States of America the One Hundred and Fortieth.

(Sgd.) Jeremiah Neterer, 11691
United States District Judge.

Service of this Citation and a copy thereof is hereby admitted this 19th day of November, 1917.

E. L. Skeel, Donworth & Todd &
Philip Farnsworth,
Solicitors for Kilbourne & Clark
Manufacturing Company.

11692 (STIPULATION IN RE TRANSCRIPT ON APPEAL.)

It is hereby stipulated and agreed, subject to the approval of the Court, that the portions of the proceedings, record and exhibits in the above entitled suit to be incorporated in and to form the transcript on plaintiff's appeal from the final decree entered herein on or about the 16th day of July, 1917, shall consist of the following:

1. Bill of complaint.

2. Second amended answer verified February 24, 1916, and filed.

11693 3. Opinion of Court as printed in 239 F. R. pp. 328-355.

4. Decree appealed from.

5. Appeal papers.

6. The following portions of the opening statement of counsel: Provided that either party may object to the competency, relevancy or materiality thereof on the appeal herein.

Page 1, lines 1-23.
" 4, " 14 to and including the word "invention", line 28.
Page 60, line 1, the words "Mr. Skeel".
Page 60, lines 14-29.
11694 " 63, " 17-21,
" 69, line 25, beginning with the words "In Mr. Betts' ", to and including line 3, page 71.
" 75, line 11, to and including line 25.

7. The agreed statement of the evidence consisting of the stenographers' minutes of the evidence and proceedings, with the clerical corrections agreed upon, comprising five volumes on file herein, including such of the reports, charts, photographs, exhibit diagrams, drawings and sketches forming a part of the text of and incorpo-

 11695

rated in said evidence, without reducing said evidence to narrative form, but condensing the same and the said minutes by omitting the following portions thereof, appearing on the following numbered pages of said minutes:

Vol. 1, Plaintiff's Prima Facie Evidence:

Pages 1-77, Opening Arguments, except the portions noted in paragraph 6 above.

Pages 1-4, inclusive.

Page 5, line 2 to page 16 inclusive.

Page 38 to line 16 page 40.

Page 205, lines 15-30. 11696

Page 215, line 16, to line 8, page 216.

Page 225, line 24, to line 2, page 226.

Page 244, lines 1-4; 11-19.

Page 254, line 12 to line 17 page 256.

Pages 257-8.

Page 290, lines 3-8.

Pages 302-315.

Page 382, lines 16-25.

Page 418, lines 13-18.

Page 424, lines 3-12.

Page 429, lines 1-17.

Page 430, line 23 to page 433 inclusive. (Owing to 11697
this omission, there shall be inserted in the last question on page 440, line 25, after the words "the Lodge Patent", the following: "page 2, lines 57 to 67".

Page 442, lines 1-18.

Page 455, line 29 to page 457, line 2.

Vol. 2, Defendant's Replying Evidence:

Page 458, line 27, to line 13, page 459.

Page 460, line 20 to line 15, page 468.

Page 471, lines 12-13; page 472, lines 1 and 2.

Page 483, line 29 to line 10, page 484. (Mr. Baker's deposition is printed in full elsewhere).

11698

Page 523, lines 7-13.
Page 553, line 27 to page 556 inclusive.
Page 557 to line 4 page 559.
Page 604 line 8 to page 614.
Page 614 line 17 to line 2 page 616.
Page 951 line 14 to page 953 line 11.
Page 1008 lines 10-30.
Page 1009 lines 17-30 to line 30 page 1010.
Page 1052 line 19 to line 12 page 1053.
Page 1111 line 17 to line 23 page 1112.

Vol. 3, Defendant's Replying Evidence:

11699 Page 1231 line 17 to page 1232 line 2.
Page 1275 line 2 to page 1276 inclusive.
Page 1385 lines 19-25.
Page 1388 line 14 to line 2 page 1389; lines 7-13; 29 and 30.
Page 1390 lines 1-2.
Page 1402 lines 6-8.
Page 1409 lines 3-29.
Page 1422 lines 14-21.
Page 1425 lines 16-23.
Page 1455 lines 22-26.
Page 1456 lines 2-8.
Page 1457 lines 2-9.
11700 Page 1493 lines 3-11.
Page 1495 line 5 to page 1496 line 7.
Page 1497 lines 2-10.
Page 1508 lines 13-19.
Page 1536 line 24 to page 1537 line 12.
Page 1539 lines 6-29.
Page 1556 line 24 to and including page 1563.
Page 1576 line 8 to page 1578 line 8.
Page 1583 line 3 to page 1586 line 21.
Page 1589 line 5 to page 1592 line 2.
Page 1634 lines 21-30.

 11701

Page 1640 line 15 to and including page 1641.
Page 1805 line 3 to line 14 page 1806.
Page 1814 line 24 to page 1817 line 17.
Page 1817 line 27 to page 1819 line 2.
Page 1827 line 12 to page 1828 line 8.
Page 1866 line 14 to and including page 1869.
Page 1885 lines 7-20.
Page 1896 lines 25-30.
Page 1909 lines 6-24; 30.
Page 1910 lines 2-6.
Page 1923 lines 22-25.
Page 1927 lines 12-19. 11702
Page 1929 lines 5-10.
Page 1934 lines 2-6.

Vol. 4, Plaintiff's Rebuttal:

Page 1970 line 21 to and including line 7, page 1971.
Page 1988 lines 10-13.
Page 2006 lines 14-18.
Page 2043 line 16 to page 2044 line 7.
Page 2053 line 28 to page 2055 line 13.
Page 2058 lines 17-20.
Page 2059 lines 2-3.
Page 2060 lines 10, 11, 13, 14.
Page 2063. 11703
Page 2106 line 27 to page 2109 line 6.
Page 2109 line 10 to line 10 page 2112.
Page 2115 lines 6-24.
Page 2131 line 15.
Page 2134 lines 3-21.
Page 2166 lines 3-11.
Page 2201 lines 5-7.
Page 2206 lines 24-25.
Page 2207 lines 5-13.
Page 2220 line 28.

11704 Stipulation In Re Transcript on Appeal.

Page 2226 line 4.
Page 2230 line 16 to page 2231 line 30.
Page 2240 lines 19-30.
Page 2250 line 24 to page 2251 line 7.
Page 2253 lines 1-9.
Page 2256 lines 23-27.
Page 2264 lines 3-6; 10-14.
Page 2269 line 27 to page 2270 line 3.
Page 2282 line 11 to page 2286 line 7.
Page 2289 line 15 to page 2290 line 15.
Page 2293 lines 12-22.
11705 Page 2298 line 28 to page 2299 line 5.
Page 2304 lines 18-27.
Page 2305 lines 26-30.
Page 2306 lines 12-14.
Page 2332 line 10.
Page 2353 lines 3-17.
Page 2358 lines 2-4.
Page 2359 line 2 to line 17 page 2362.
Page 2395 lines 23, 25-29.
Page 2446 line 26 to page 2448 line 10.
Page 2448 lines 15-27.
Page 2493 to page 2494 line 11.
Page 2528 line 22 to page 2529 line 3.
11706 Page 2570 lines 9-11.
Page 2585 lines 12-30.
Page 2589 lines 7-10.
Page 2655 lines 7-10.
Page 2661 lines 23-25.
Page 2699 line 22 to page 2701 line 9. Lodge British patent 11,575.
Page 2720 lines 7 and 8.
Page 2737 line 24 to line 2 page 2738.
Page 2744 line 24 to line 4 page 2745.
Page 2745 lines 18-25.

Page 2754 lines 23-26.

Page 2767 line 27 to page 2799 inclusive. Plaintiff's argument and affidavits on motion for adjournment and leave to introduce evidence as to Simpson Mercury Valve, except lines 5-30 page 2793.

Page 2800 line 27 to line 10 page 2810. Argument and ruling on motion to strike out Lodge British patent 11,575.

Page 2810 line 11 to page 2872 inclusive. Discussion, defendant's argument and affidavits in opposition to motion for adjournment and to introduce evidence. 11708

Page 2876 line 7 to and including line 13 page 2882. Discussion in re date of adjournment.

Page 2882 line 22 to page 2887 line 3.

Page 2887 lines 14 to 20.

Page 2893 lines 19-23.

Vol. 5, Plaintiff's Evidence (in re Simpson Mercury Valve).

Page 2897 line 22 to line 5 page 2898.

Page 2909 line 30 to page 2910 line 18.

Page 2912 lines 4-15.

Page 2913 line 17 to page 2919 line 3; line 9 page 2920 to line 26 page 2921; line 29 page 2924 to line 2 11709
page 2925; line 17 page 2925; line 19 page 2928 to line 10 page 2929; line 10 page 2930 to line 19 page 2934; lines 2-13, 22-30 page 2935; line 1 page 2936.

Page 3065 lines 6-12.

Page 3074 line 27 to page 3081 line 14; lines 18-30 page 3081.

Page 3094 lines 8-14.

Page 3107 lines 2-11.

Page 3108 lines 7-10, 19.

11710 Stipulation In Re Transcript on Appeal.

Page 3120 line 20.
Page 3121 line 24 to page 3122 line 7.
Page 3124 lines 17-19.
Page 3126 line 26 to page 3128 line 12.
Page 3137 lines 2-6.
Page 3151 lines 13-17.
Page 3158 line 14 to page 3159 line 18.
Page 3160 lines 19-21.
Page 3161 lines 1-8; line 18 to pages 3162 line 8.
Page 3173 line 9.
Page 3195 lines 8-18.
11711 Page 3197 lines 16-21.
Page 3200 lines 13-20 and 28-30.
Page 3201 lines 1-5.
Page 3202 lines 24-28.
Page 3203 lines 9-10 and 18-20.
Page 3204 lines 12-18.

Vol. 5 Defendant's Surrebuttal Evidence:

Page 3218 lines 11 and 12.
Page 3219 lines 5-9; 18-29.
Page 3234 lines 1-3.
Page 3240 line 27 to page 3241 line 13.
Page 3254 lines 7-22.
11712 Page 3256 lines 13-15.
Page 3257 line 29 to line 3 page 3258.
Page 3258 line 23 to line 5 page 3259.
Page 3264 line 28 to line 6 page 3265.
Page 3268 line 14 to line 15 page 3269.
Page 3275 line 25 to line 2 page 3276.
Page 3279 line 30 to line 2 page 3280.
Page 3281 lines 2-6; 15-19.
Page 3332 line 8 to page 3334 line 3.
Page 3334 line 22 to page 3336 line 2.
Page 3336 lines 20-29.

 11713

Page 3352 line 23 to page 3353 line 19.
Page 3373 lines 27-30.
Page 3374 lines 18-26.
Page 3419 line 27 to page 3420 line 6.
Page 3422 line 30 to page 3423 line 11.
Page 3429 line 12 to page 3430 line 19.
Page 3431 lines 10-13.
Page 3436 lines 6-7.
Page 3443 line 22 to page 3444 line 8.
Page 3444 line 29 to page 3445 line 2.
Page 3447 line 1 to page 3448 line 19.
Page 3449 lines 1-2; 12-15. 11714
Page 3449 line 30 to page 3450 line 12.

Vol. 5 Plaintiff's Reply Evidence to Defendant's Surrebuttal.

Page 3463 line 18 to page 3464 line 26.
Page 3505 lines 9-10; 21-27.
Page 3506 line 25 to page 3507 line 2.
Page 3508 line 10 to page 3509 line 13.
Page 3509 lines 26-30.
Page 3516 lines 9-13.
Page 3526 line 24 to page 3527 line 6.
Page 3528 lines 6-11; 24-26.
Page 3529 lines 2-16. 11715
Page 3531 line 19.
Page 3534 line 28.
Page 3544 lines 11-12.
Page 3554 lines 8-12.
Page 3564 lines 10-15.
Page 3574 lines 16-27.

Vol. 5 Defendant's Evidence in Reply to Plaintiff's Receiver Tests.

Page 3600 line 28 to page 3601 line 4.
Page 3603 line 29 to page 3604 line 2.

11716 Stipulation In Re Transcript on Appeal.

Page 3604 line 29 to page 3605 line 22.
Page 3606 lines 26-27.
Page 3609 lines 21-26.
Page 3610 lines 26-30.

Said omissions shall be noted by parenthetical remarks in the transcript where necessary to explain the context; such remarks to be approved by both parties.

8. Defendant's replying depositions, without condensation or reduction to narrative form, taken in New York before adjournment and compris-
11717 ing the following:
Joseph B. Baker
John Stone Stone
Frederick M. Sammis

9. Plaintiff's rebuttal depositions, without condensation or reduction to narrative form, taken in New York before adjournment and comprising the following:
William J. Kinsley
David Sarnoff
John Bottomley
John W. Griggs

11718

10. Plaintiff's depositions, without condensation or reduction to narrative form, taken in Boston and comprising the following:
E. L. Chaffee
J. H. Morecroft
J. G. Coffin
Charles R. Cross

11. DEFENDANT'S SURREBUTTAL DEPOSITIONS, WITHOUT CONDENSATION OR REDUCTION TO NARRATIVE FORM, TAKEN IN NEW YORK AND COMPRISING THE FOLLOWING:

Emil J. Simon
John Stone Stone
Greenleaf Whittier Pickard.

Following drawings to be inserted in the minutes where referred to:

T1, containing Stone diagrams 1, 2 and 3;
T2, " " " 4, 5 " 6; 11720
T6, being diagram from Zenneck book
Defendant's exhibits G. W. P. 66, 67 and 68.

12. The following portion of the closing argument of Mr. Betts:

Page 406 line 18 to page 407 line 6.

It being further stipulated that any part of the opening and closing arguments of counsel in the court below may be made use of by either side in their briefs in the Circuit Court of Appeals; all of which shall be subject to any objection of counsel to the competency, materiality or relevancy thereof.

11721

13. ALL OF THE ASSESSORS', PLAINTIFF'S AND DEFENDANT'S EXHIBITS OFFERED OR RECEIVED IN EVIDENCE AT THE TRIAL AND ON THE TAKING OF THE FOREGOING DEPOSITIONS OUT OF COURT, WHICH WERE READ, OFFERED OR RECEIVED IN EVIDENCE AT THE TRIAL.

IT IS FURTHER STIPULATED AND AGREED, subject to the approval of the Court, that the originals of all of said exhibits offered by both parties shall, as such, be transmitted by the Clerk of this Court to the Circuit Court of Appeals for the Ninth Circuit as physical exhibits and form a part of said transcript of record on appeal, but that only the following need be printed as part of the said transcript to be printed or reproduced.

PLAINTIFF'S EXHIBITS.

Ex. No.

1. Chart of Marconi Reissue, Lodge and Marconi tuning patents, incorporated in minutes opposite page 346.
2. Alameda Chart, incorporated in minutes opposite page 243.

11723 3. Lodge Patent No. 609,154, omitting the certification and grant.

4. Marconi Patent No. 763,772, omitting the certification and grant.
8. Series of diagrams F. N. W. 1 to F. N. W. 12 inclusive, incorporated in minutes between pages 28 and 208.
9. "Achilles" chart, incorporated in minutes opposite page 230.
10. Weagant drawing R. A. W. 1, Marconi apparatus Smith Building, incorporated in minutes opposite page 260.

11724 11. Weagant sketches R. A. W. 2 and 2a, incorporated in minutes opposite pages 298 and 301.

12. Marconi Reissue Patent No. 11,913.
20. Such portions of the "Proceedings of the Institute of Radio Engineers" of "April, 1915", as contain an article by Frederick A. Kolster entitled "The Effects of Distributed Capacity of Coils used in Radio Telegraph Circuits."
27. Taylor blueprints and drawings T. 1 to T. 10 inclusive, incorporated in minutes between pages 2008 and 2045.

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29. Instructions issued by Marconi's Wireless Telegraph Company, Limited, for use in Marconi tuned wireless apparatus.
38. H. Kingsley photos of Stone-Baker letters.
39. I. Sarnoff drawing, Early Marconi Tuned Transmitters and Receivers.
40. Marconi British patent No. 7777.
41. Justice Parker's decision (being embraced in J-4, plaintiff may omit if it so elects.)
42. Justice Eves' decision.
43. Translated French decision.
45. R. A. W. chart No. 3, incorporated in minutes 11726
opposite page 2202.
46. F. N. W. diagram No. 14, incorporated in minutes opposite page 2327.
47. F. N. W. diagram No. 15, incorporated in minutes opposite page 2352.
47A. F. N. W. diagrams Nos. 16-25, incorporated in minutes between pages 2386 and 2760. 16-31-17-51, 18-25 inc. have 44.
50. Chart of National Electric Signaling Company apparatus, incorporated in minutes opposite page 2510.
51. Such portions of "The Electrician" of "September 8, 1911", as contain an article by G. W. 11727
O. Howe, entitled "Recent Developments in Radio Telegraphy".
52. Chart of Marconi transmitting tunes, incorporated in minutes opposite page 2656.
53. Chart of Marconi receiving tunes, incorporated in minutes opposite page 2661.
54. Chart of Atlantic Communication apparatus.
59. Such portions of the "Proceedings of the Institute of Radio Engineers" of "April, 1913", as contain the paper read by Dr. Lewis W.

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Austin, entitled "The Relation between Effective Resistance and Frequency in Radio Telegraphic Condensers", viz., pages 35-39.

66A. Drawings Figures 1 and 2 Marconi 1896 apparatus.

67. A to D, consisting of the following, introduced in the taking of plaintiff's Boston depositions:
   A. Chaffee diagrams Nos. 1 to 9 inclusive.
   B. Morecroft diagrams Nos. 1 and 2.
   C. Coffin diagrams Nos. 1 and 2.
   D. Cross diagrams Nos. 1-4 inclusive.

11729 68. Diagram defendant's Simpson Mercury Valve, incorporated in minutes opposite page 2939.

69. List of patents and publications introduced in evidence in Marconi *vs.* National Electric Signaling Company, incorporated in minutes pages 3083-3088.

70. F. N. W. diagram No. 26, incorporated in minutes opposite page 3012.

71. R. A. W. diagram No. 4, incorporated in minutes opposite page 3183.

73. F. N. W. diagram No. 30, incorporated in minutes opposite page 3559.

74. Resonance curve, Chaffee test, incorporated in
11730 minutes opposite page 3574.

75. Waterman's comparison of references and defenses, National case, incorporated in minutes page 3583-3590, omitting answers in National case.

Stone Patents Nos. 714, 832, 714, 833 and 767,975, introduced typewritten page 56 Mr. Stone's second deposition.

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DEFENDANT'S EXHIBITS.

2. F. N. W. diagram No. 13, incorporated in minutes opposite page 455.
4. The following from Binder F (introduced on Mr. Baker's and Mr. Stone's first deposition, as defendant's exhibits Nos. 1-10):
   - F1. Stone-Baker letter July 22, 1899.
   - F2. Stone-Baker letter June 30, 1899.
   - F3. Stone-Baker letter July 18, 1899.
   - F4. Baker affidavit of March 18, 1901.
   - F5. Photograph of Baker affidavit of January 29, 1904. 11732
   - F7. Baker-Brown letter June 3, 1903.
   - F10. Attested statement of G. A. Higgens executed January 4, 1904.

5 and 31. The following from Binder R:
   - R1. Certified copy file-wrapper and contents Stone patent No. 714,756.
   - R2. Certified copy file-wrapper and contents Stone Patent No. 714,831.
   - R4. Stone Patent No. 767,984.
   - R5. Stone Patent No. 767,990.

6. Charts G. W. P. Nos. 2-64 inclusive, incorporated in minutes between pages 494-930; 1484-1717; 11733 1818-1872; 1887-1905.
7. Photograph of page of Pickard diary.
10. Kolster Bureau of Standards Report and drawings attached thereto, and oscillographic records, curves and drawings attached thereto.
11. Chart Kolster curve SS "Admiral Evans".
12. Kolster charts K1-K16 inclusive, incorporated in minutes pages 1125-1166; 1282-1290; 1365-1377.
14. Simpson formula.
15. Charts E. W. S. 1-11 inclusive, incorporated in minutes pages 1182-1227.

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16. Simpson charts and drawings F. G. S. 1-11 inclusive, incorporated in minutes pages 944-1067.

17. Thompson charts R. E. T. 1 and 2, incorporated in minutes between pages 1615-1621.

26. The following from Binder J:

J4. Copy of Justice Parker's Opinion and summary of case.

27. The following from Binder M:

M1. File-wrapper and contents of Tesla Patent No. 645,576.

11735 M2. File-wrapper and contents of Tesla Patent No. 649,621.

28. The following from Binder G:

G1. Such portions of "The Electrician" of London, "June 11, 1909", as contain Fleming article entitled "The Telefunken or Quenched Spark Discharger".

G2. Such portions of "The Electrician" of "Nov. 10, 17 and 24, 1911" as contain article entitled "The Telefunken System of Wireless Telegraphy".

29. The following from Binder L or H (introduced as Defendant's Exhibits 11-15 in taking Sammis'

11736 deposition):

L11.	Sammis Sketch Marconi Early Receiver.	To be inserted in minutes when referred to.
L12.	Sammis Sketch Marconi Magnetic Receiver.	
L13.	Sammis Sketch Plain Aerial.	
L14.	Sammis Sketch Marconi Coupled Circuit Transmitter.	

L15. Marconi Nobel Prize Lecture delivered before Royal Academy of Science "on 11th December, 1909".

30. The following from Binder E:
   - E3. File-wrapper of Braun Pat. No. 797,169.
   - E4. " " " " No. 797,544.
   - E5. Braun British Pat. No. 1862 of 1899.

32. The following from Binder H:
   - H2. File-wrapper of Lodge U. S. Patent No. 609,154.
   - H3. Lodge corresponding British Patent No. 11,575 of 1897.
   - H5. Judge Parker's Opinion refusing to extend Lodge British tuning claims.

33. The following from Binder I: 11738
   - I–1. Marconi U. S. Patent No. 586,193.
   - I–2. File-wrapper Marconi U. S. Patent No. 627,-650.
   - I–3. File-wrapper Marconi U. S. Patent No. 647,-007.
   - I–4. File-wrapper Marconi U. S. Patent No. 647,-008.
   - I–5. File-wrapper Marconi U. S. Patent No. 647,-009.
   - I–6. File-wrapper Marconi Patent No. 668,315.
   - I–7. Translation French Patent No. 283,521 and addition of June, 1899.
   - I–8. Marconi British Patent No. 12,039 of 1896. 11739
   - I–9. Marconi British Patent No. 12,326 of 1898.
   - I–10. Marconi British Patent No. 6982 of 1899.
   - I–11. Marconi British Patent No. 25,186 of 1899.

34. The following from Binder K:
   - K1. Stone U. S. Patent No. 577,214.
   - K2. Stone U. S. Patent No. 638,152.
   - K3. File-wrapper Stone U. S. Patent No. 726,368.
   - K4. Hutin & LeBlanc U. S. Patent No. 838,545.
   - K5. Bedell U. S. Patent No. 715,537.

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36. The following from Binder A:
A5. Tesla U. S. Patent No. 645,576.
A7. Tesla U. S. Patent No. 649,621.
A10. Article "Propagation of Long Electric Waves", by M. I. Pupin.
A12. Page 1636 from "Illustrated Official Journal of Bridgments" (Lodge Figure 4, British Patent).
A22. Pupin Paper "Possibilities of Wireless Telegraphy".

37. The following from Binder B:
11741 B2. Translation from Wiedemann's Annalen "1890", No. 12, pp. 850-870, of "An Investigation of Electrical Resonance Phenomena", by Ernest Leecher.
B3. Article by Oliver Lodge entitled "Effect of a Condenser Introduced into an Alternating Current Circuit", "The Electrician" "April 24, 1891", page 762.
B5. Translation of article "Experimental Determination of the Speed of Propagation of Electro Magnetic Waves", by Blondlet, "Journal der "Physik", "1891", pp. 549-561.
11742 B6. Article entitled "Radiation of Electric Energy", by Trouton, from "The Electrician", "January 15, 1892", pages 280, 281, "January 22, 1892", pages 301-303.
B7. Article entitled "Electro-Magnetic Vibrations", by Fitzgerald and Garrett, from "The Electrician", "January 29, 1892", pages 329-330; 333-334.
B14. Article entitled "Practical Aspects of Low Frequency of Electrical Resonance," by M. I. Pupin, "Transactions A. I. E. E." "May 17, 1893", pp. 370-379.

B15. Article entitled "Electrical Oscillations", etc., by Pupin, "American Journal of Science", "1893", pages 325-334.

B18. Article entitled "Work of Hertz", by Lodge "The Electrician" "June 8, June 15 and June 22, 1894".

B20. "Elementary Lessons in Electricity and Magnetism", "1895", pages 554-558; 561-563, by S. P. Thompson.

B21. Translation "Studies in Electrical Resonance", by Domlip and Kolacek, "Annalen der Physik", Vol. 57, pp. 731-750. 11744

B22. Article "Electrical Oscillations", Poincare, Review, by Pupin, in "Science" "January 26, 1905", page 106.

B23. Paper "On the Phenomena of Electrical Oscillations in Tesla's Investigation", by A. Oberbeck. "Annalen der Physik und Chemie", "1895", Vol. 55, p. 623.

B26. Article "Willung Improved Induction Coils and Variable Condensers", from "The Electrician" "June 17, 1896".

B31. Article "Electrical Resonance", "The Electrical Review", page 778, "June 4, 1897".

B33. Article "Signaling Through Space Without Wires", by Preece, "The Electrician" "June 11, 1897", pages 217-218. 11745

B34. Editorial "Wireless Telegraphy", "Electrical World" "July 10, 1897", pages 29-30.

B35. Translation of Paper entitled "Resonance", by Della Riccia, Revista di Artigleria, "1897", Vol. 4, pages 204-206.

B37. Paper by Northup entitled "Some Experiments on Induction with Currents of High Frequency at Long Distance", "Electrical World", Vol. 30, "1897", pages 732-733, 750, 755-757.

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B41. Editorial entitled "Hertzian Telegraphy" at Physical Society", "The Electrician" "January 28, 1898, pages 452-453.

B42. Article entitled "Electric Signalling without Wires", by Lodge, "Electrical Engineer", "February 4, 1898", pp. 147-149.

B43. Article "Prof. Lodge on Electric Signalling Through Space without connecting Wires", "Electrical Engineer" "February 24, 1898", pp. 217-218.

B47. Article "Oscillatory Currents and Some of
11747 their Phenomena", by Prof. E. F. Northup, "Electrical World", Vol. 31, pp. 314, 524-526; 584-585; 607-610; 674-676; 710-711.

B48. Translation of paper by Della Riccia on "Electro-Magnetic Waves and Wireless Telegraphy", "Bulletin Association of Electrical Engineers", etc., No. 4, Vol. IX, pp. 161-165.

B50. Article entitled "Lodge's System of Syntonized Wireless Telegraphy", "Electrical Review", Vol. 33, No. 10, pp. 148, 149.

B54. Paper "Improvements in Magnetic Space Telegraphy", by Lodge, "Journal of Institution of Electrical Engineers", pp. 799-851.

11748 B55. Article entitled "High Frequency Oscillators for Electro Therapeutic and other Purposes", "Electrical Engineer", Vol. 26, No. 550, pp. 477-481.

B57. Translation of paper entitled "Electrolytic Cells with Platinum Electrodes", by Pupin, "Zeitschrift für Electrotechnik", No. 49, pp. 614, 615.

B70A. Article entitled "The Experiments of Prof. Hughes on Ether Telegraphy", by J. Munro, "Electrical Review", Vol. 44, pp. 883, 884.

B72. Article entitled "A New Marconi Patent", "Electrical World and Engineer", "July 22, 1899", p. 119.

B81. Paper "Recent Progress in Wireless Telegraphy", by Marconi, "The Electrician", "February 9, 1908", pp. 552, 553, 555-557.

38. The following from Binder C:

C17. Pupin U. S. Patent No. 519,347.

C19. British Patent to Tesla No. 20,981 of 1896.

C40. Pupin U. S. Patent No. 713,045.

C59. Braun U. S. Patent No. 797,169.

C62. Braun British Patent No. 1862 of 1899. 11750

C69. Braun U. S. Patent No. 797,544.

C70. Braun British Patent No. 12,420 of 1899 (?).

C79. Pupin U. S. Patent No. 640,516 (?).

C89. Stone U. S. Patent No. 726,476 (?).

All of group 8 in Binder C containing Marconi Patents as follows:

British Patent No. 12039.
U. S. Patent No. 672650.
U. S. Patent Nos. 647007, 647008, 647009.
British Patent No. 25186.
U. S. Patent No. 668315.
U. S. Patent No. 676322. 11751

All of the Tesla patents shown in Group 11 in said Binder C including U. S. Patent No. 685012, U. S. Patent 685954, U. S. Patent 685953, and any other Tesla Patents shown in said group.

45. "The Progress of Wireless Telegraphy", by G. Marconi, "Transactions of New York Electrical Society", No. 15, "1912".

46. Paper "The Progress of Electric Space Telegraphy", by Marconi, "Royal Institution of Great Britain", "June 13, 1902".

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47. Lecture by Marconi on "Recent Advances in Wireless Telegraphy", delivered at the Royal Institution "on 3rd March, 1905".
48. Lecture "Synthetic Wireless Telegraphy", by Marconi, "Journal of Society of Arts", "May 17, 1901", pp. 505-517.
49. Article "Wireless Telegraphy", by Marconi, "Electrical Review, and Western Electrician", "February 5, 1910", pp. 283-285.

50. Chaffee diagram No. 10. 51. Chaffee diagram No. 11. 11753 52. Cross diagram No. 4. 53. Morecroft diagram No. 3.	To be inserted in minutes when referred to.

54. Diagram F. N. W. No. 27, incorporated in minutes opposite page 3117.
55. Diagram F. N. W. No. 28, incorporated in minutes opposite page 3124.
56. Diagram F. N. W. No. 29, incorporated in minutes opposite page 3139.
64. Zenneck charts and blueprints marked Z1 to Z18 to be inserted in minutes when referred to.
65. File-wrapper and contents of Stone Patent No. 767,975.

11754 66. Assessor's chart of circuits of defendant's standard receiver.
67. Assessor's chart of circuits of defendant's standard receiver with different secondary coil. Assessors' Exhibits, Charts, Etc.

Magnusson-Marriott, Assessors' Charts Nos. 1-4 inclusive, inserted in minutes opposite page 1873.

Assessors' chart No. 5, incorporated in minutes opposite page 2084.

Assessors' chart No. 6, incorporated in minutes opposite page 2091.

Assessors' resonance curves of assessors' tests Nos. 17 and 18, Pickard-Lodge apparatus, incorporated in minutes opposite page 1873.

Stipulation In Re Transcript on Appeal. 11755

It is further stipulated, subject to the approval of the Court, that either party may print, reproduce or refer to in its brief in the Court of Appeals, and the Court shall consider any portion of any original exhibit transmitted as a physical exhibit as part of said transcript, and which is not to be printed or reproduced as part of the printed transcript under this stipulation, and in the event that this stipulation shall not be approved by the Court, either party may designate such additional exhibits or portions thereof it desires printed.

It is further stipulated, subject to the approval of the Court, that the Clerk of this Court shall certify to 11756
the Circuit Court of Appeals for the Ninth Circuit the foregoing portions of the pleadings, proceedings, and evidence, and shall transmit the original exhibits herein as the said transcript on said appeal as agreed upon by the parties hereto, said original exhibits to be returned to this Court after the hearing and determination of the case in the said Court of Appeals.

Dated November 19, 1917.

Sheffield & Betts,
E. C. Hughes,
Plaintiff's Solicitors.

11757

E. L. Skeel,
Defendant's Solicitor.

Donworth & Todd,
Philip Farnsworth
& Roberts, Wilson & Skeel,
Of Counsel.

The foregoing stipulation is hereby approved, and the Clerk of this Court is ordered to certify the record on the appeal and to transmit the original exhibits herein accordingly, without requiring any *praecipe* or any further approval of said record by this Court.

Dated November 19, 1917.

(Sgd.) JEREMIAH NETERER,
United States District Judge.

The foregoing stipulation and order are approved.

Dated November 14, 1917.

(Signed) WM. B. GILBERT,
United States Circuit Judge.

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IN THE UNITED STATES CIRCUIT COURT OF APPEALS. 11761

FOR THE NINTH DISTRICT.

MARCONI WIRELESS TELEGRAPH COMPANY OF AMERICA,
Appellant,

vs.

KILBOURNE & CLARK MANUFACTURING COMPANY, 11762
Appellee.

No. 71

Order Extending Time.

IT IS HEREBY ORDERED that the time of the appellant herein to file the record and docket this cause on appeal in the United States Circuit Court of Appeals for the Ninth Circuit may be and the same hereby is extended to and including March 15, 1918.

Dated: January 5th, 1918.

JEREMIAH NETERER.
Judge.

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